AMERICAN SCIENTIST

JUNE, 1964

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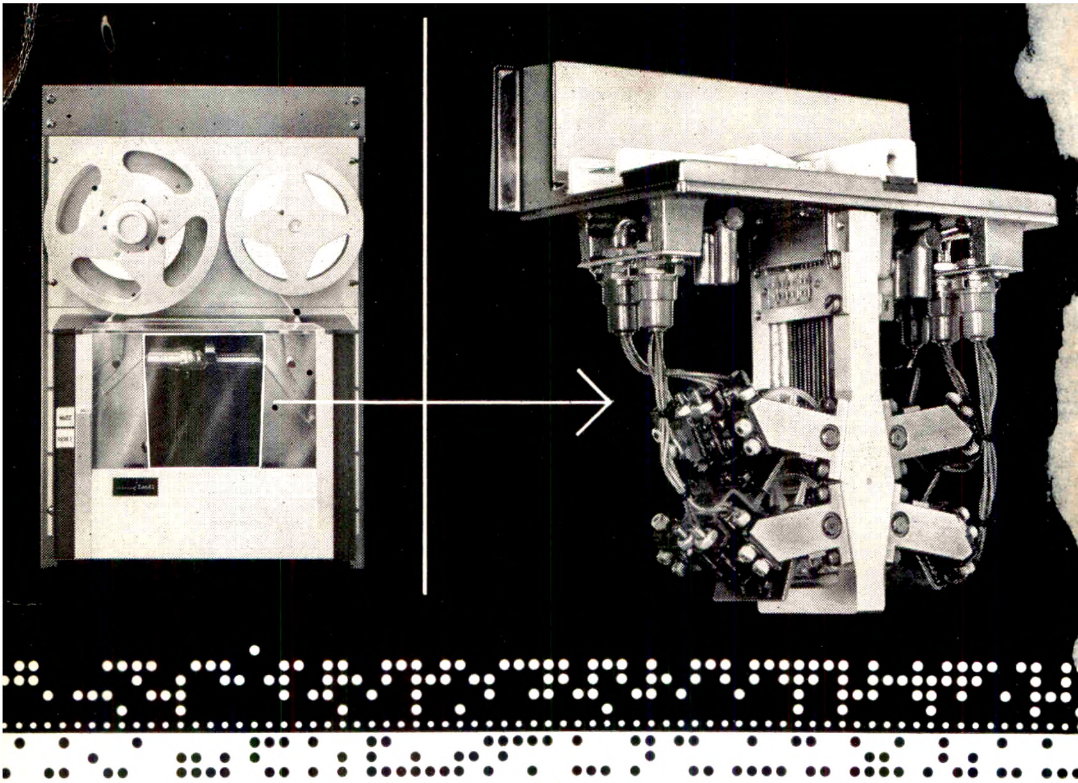

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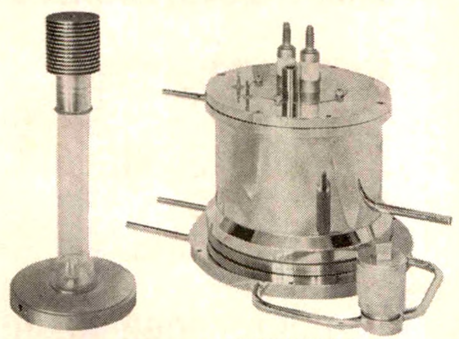

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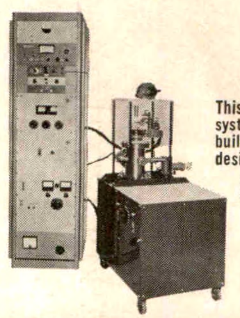

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VOLUME 52 JUNE 1964 NUMBER 2

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CONTRIBUTORS

Through the courtesy of The American Philosophical Society we are permitted to reproduce the lecture, *Atom to Adam*, given by Nobel Laureate Melvin Calvin, of the Department of Chemistry in the University of California, Berkeley, at the November 1963 meeting of the Society. The lecture stems from his research interests in the area of photosynthesis and the organization of complex molecules from the primal materials in the universe—hydrogen, methane, water, and ammonia. Genevieve J. Calvin is the wife of M. Calvin. Her contribution to the present paper reflects her broad interest and concern with the impact of scientific development on our society—locally, nationally, and globally.

A Sigma Xi-RESA National Lecturer for 1962–63 is the Florence Pirce Grant University Professor of Psychology at Brown University, where he graduated A.B. in 1933. As a Rhodes Scholar, 1935–38, and with subsequent studentships, he received his B.A. in Physiology (1937) and his Ph.D. in Psychology in 1939 from Oxford and Cambridge Universities, respectively. A member of the National Academy of Sciences, his research interests are in the area of physiological psychology and sensory processes. In 1960 he received the Howard Crosby Warren Medal for his researches in experimental psychology.

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Robert G. Jahn, *Electric Propulsion* 207

The author of this article took undergraduate and graduate degrees in Princeton University (1951–55). He became instructor in physics at Lehigh University, 1955–56, and Assistant Professor in 1956. In 1958, he transferred to the Jet Propulsion Laboratory of California Institute of Technology as Assistant Professor, returning to Princeton University's Department of Aeronautical Engineering in 1962. His research interests include shock tube, boundary layer phenomena, gas dynamics, and pulsed plasma propulsion. In this article he outlines alternative processes for producing space machines requiring the thrusts necessary to attain man's planetary objectives.

Owen Gingerich, *The Computer Versus Kepler* 218

An astrophysicist at Smithsonian Astrophysical Observatory, Cambridge, Mass., specializing in the computation of theoretical stellar atmospheres, addresses himself here to the use of the computer in the solution of Kepler's calculations on the orbit of Mars. He shows that both Kepler and he expected to solve the problem in two weeks. Kepler took at least four years. Programming for the computer took two weeks and the solution was then obtained in less than 10 seconds. The author has taught astronomy at the American University of Beirut (1955–58) and at Wellesley College (1958–59).

Glenn L. Jepsen, *Riddles of the Terrible Lizards* 227

The author, Sinclair Professor of Geology and Curator of Paleontology at Princeton University, with a special field of research in Early Tertiary Mammals, has made dinosaurs one of his many avocations. His essay reflects the abundance of his knowledge in this field and demonstrates his capacity in literate science-writing. We are indebted to the *Princeton Alumni Weekly* for permission to present to a wider audience, in a somewhat abbreviated form, this article, intended initially to interest the Princeton alumni in one phase of geologic-paleontological research.

Edwin H. Land, *The Retinex* 247

The William Procter Prize Address for 1963 was given by Edwin H. Land, known throughout the world for his applications of polarized light and as inventor of the Polaroid Camera, yielding finished photographs immediately after exposure, initially in black and white, and now in color. Recipient of numerous medals and awards, both here and abroad, a member of the National Academy of Sciences, he combines high competence as a corporation executive with continued dedication to and direction of scientific research. The lecture discusses a new approach to the problem of color perception and its significance in neural physiology. American Scientist gratefully acknowledges the gift by Dr. Land of the color illustration. Address: Polaroid Corporation, Cambridge, Massachusetts.

Jean M. Ingersoll, *The Australian Rabbit* 265

A graduate of Columbia University and a member of the research staff, Hudson Institute, Harmon-on-Hudson, New York is studying ecological problems in possible postnuclear environments, a project of the Office of Civil Defense. Specific historical examples of ecological devastation are being studied. The Australian Rabbit represents a major ecological disturbance produced by the introduction of the European rabbit onto the Australian continent.

JAMES H. BILLINGTON, *Perspectives—Science in Russian Culture* 274

An Associate Professor of History at Princeton teaching courses on Russian and European intellectual history, Dr. Billington was a guest lecturer at the University of Leningrad in 1961, and deals more fully with problems raised in this article in his forthcoming "Intellectual History of Modern Russia."

BYRON L. JOHNSON, *The Changing Role of Scientists in International Affairs* 138A

A Special Consultant to the Director of the Agency for International Development of the Department of State (Washington 25, D. C.), Dr. Johnson, has, in this capacity, traveled extensively on special and study trips. A Ph.D. of the University of Wisconsin, he was formerly Professor of Economics at the University of Denver. He was a U.S. Congressman from Colorado and also served as a special assistant to Governor Steve McNichols. This article was originally delivered as a talk to the Sigma Xi Chapter of Colorado State University, Fort Collins.

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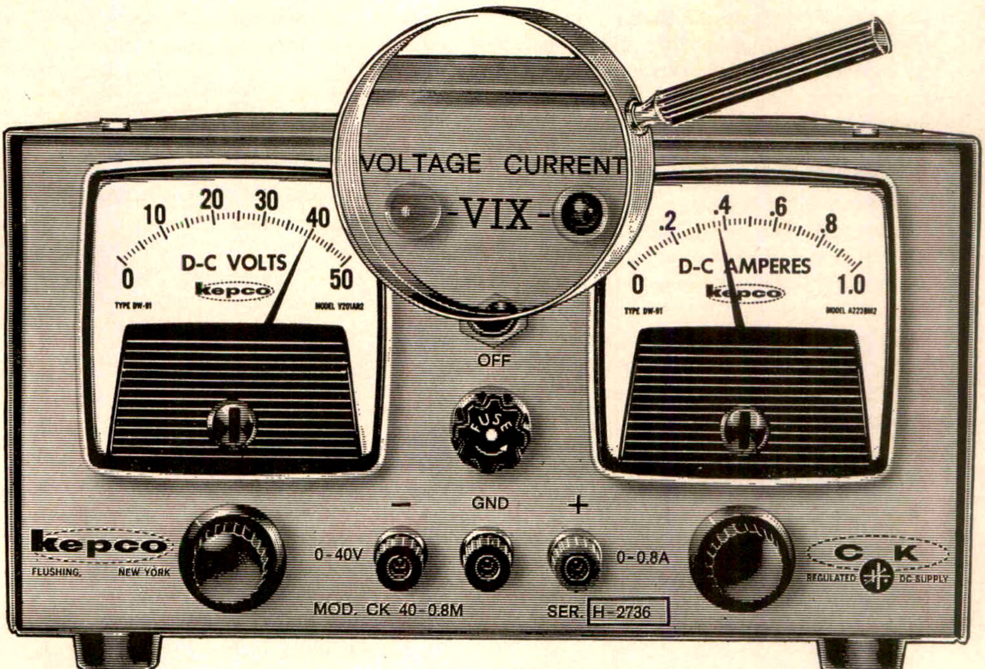

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NEWS AND VIEWS

By the Board of Editors and the Membership of the Society of the Sigma Xi and the Scientific Research Society of America, RESA

The Board of Editors is pleased to announce the appointment of Dr. Robert A. Naumann as a Consultant, in succession to Dr. Walter J. Kauzmann, who has requested release from his work for AMERICAN SCIENTIST, because of the pressure of his new responsibilities as Chairman of the Department of Chemistry at Princeton University. We extend our thanks to Dr. Kauzmann for his generous services to the Editors in selecting books suitable for review in the field of chemistry and proposing suitable persons to write these reviews. Professor Naumann will take over these duties and extend them to the areas of physics dealing with stable and radioactive isotopes, and to problems arising from their applications. Our two other Consultants, Professors George B. Field in the areas of astronomy and astrophysics and Richard O. Rouse, Jr. in psychology will continue their work as Consultants in their respective fields.

The National Executive Committee, at its Spring meeting, acted to approve several recommendations for improving a number of the Society's activities.

During the early part of 1963, the Executive Committee had authorized an audit of the Society by an outside firm of management consultants. Although specific changes were not recommended, many questions were raised together with possible courses of future action. Following the Convention of December 1963, a Committee on Long-Range Planning was appointed and this group immediately set upon the task of studying the management survey item by item.

The first activity to be examined intensively was that of the National Lectureship Program and, based upon the recommendations of the Committee on Long-Range Planning, the Executive Committee has voted that:

"1. The 1965–1966 Lecture Tours be reduced to two weeks per tour with:
   (*a*) Number of areas to remain the same (8).
   (*b*) Two lecturers per each area—one in the Fall and one in the Spring.
2. The local chapters and clubs shall be given the option of having:
   (*a*) Fall or Spring lecturer (first ten requests received complete tours).
   (*b*) Standard Lecture *or* a Research Seminar.
3. Each National Lecturer be paid an honorarium of $1000 (plus regular travel and subsistence expenses) in consideration of his willingness to give his lecture *or* conduct the research seminar at not more than *ten* different locations within a *two* week period.
4. The payment of the honorarium would be under the same requirement as at present—upon submission of manuscript to AMERICAN SCIENTIST."

At the direction of the 1962 Convention, President Rossini appointed a Committee to be known as the Committee on Criteria for, and Election to, Membership. This Committee was active throughout 1963 and it was upon the recommendations of this group that the 1963 Convention voted to create the Chapter-at-Large. Study on the major task continues but without holding up the approval of worthwhile recommendations until the final report is complete. The Executive Com-

(*Continued on page 138A*)

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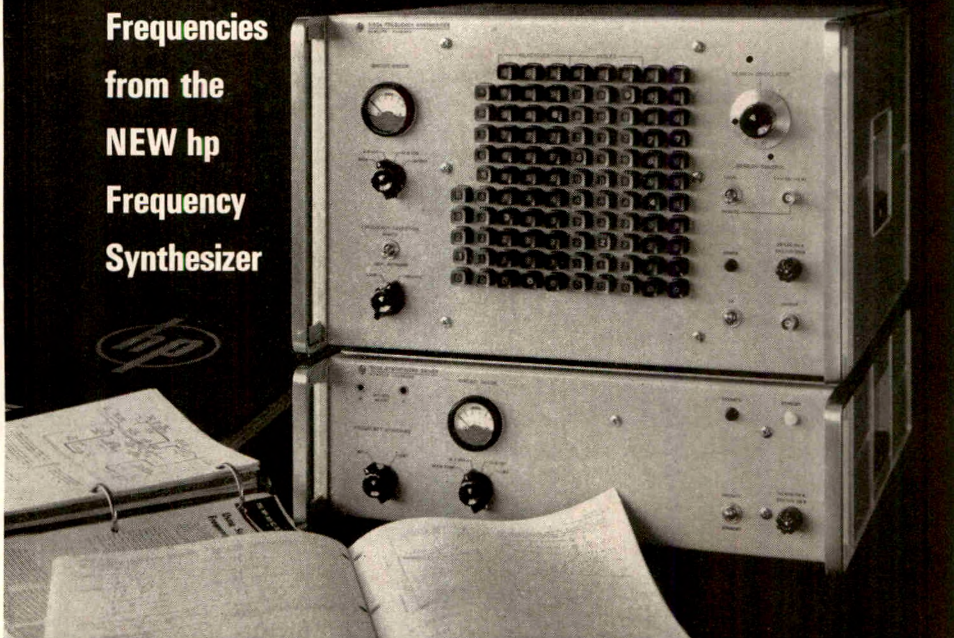

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Dr. Francis Owen Rice
University of Notre Dame, Notre Dame, Indiana
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Dr. Richard H. Jahn
Pennsylvania State University, University Park, Pennsylvania
March 1–26, 1965

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Dr. Michel Boudart
University of California, Berkeley, California
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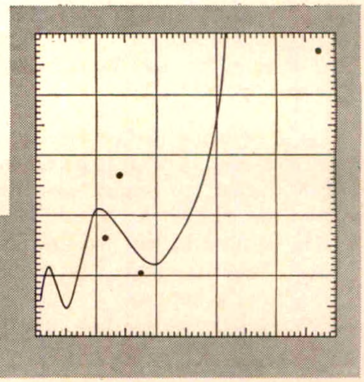

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(*Continued from page 134A*)

mittee has, accordingly, upon the recommendation of the Committee on Criteria for, and Election to, Membership, voted that:

"A standard form for the nomination of Members and Associate Members, drawn up under the direction of the National Executive Committee, be used in the process of nominating Members and Associate Members in all Chapters."

It is expected that this new form will be available for Chapters in the fall after its final approval by the Executive Committee in October.

The annual convention of The Society of Sigma Xi will be held December 29, 1964, at the Hotel Queen Elizabeth, Montreal, Canada. Dr. René Dubos of The Rockefeller Institute will give the Sigma Xi-Phi Beta Kappa address.

THE CHANGING ROLE OF SCIENTISTS IN INTERNATIONAL AFFAIRS

BY BYRON L. JOHNSON

The Scientist in Government: The business of government has always required some understanding of the role of science and engineering. Every government has required help in transportation, defense, water supply, health, and sanitation. The level of scientific insight required, however, was typically within the ken of intelligent laymen. There are always a few intelligent laymen, but only a very few who in fact mastered a great deal of scientific knowledge; in our own society, one thinks of Benjamin Franklin or Thomas Jefferson.

Usually, however, those in charge of government have had to take on faith the advice and counsel of the scientist, and have been content to give him freedom to operate without knowing fully what he was doing or why. There has always been the problem of establishing sufficient public understanding to permit and support the research and experimentation of the scientist, to finance his laboratories as well as the classrooms. But over the years, the public has seen enough that was useful, among the much that was new, to justify the public outlays and to permit scientific freedom. Yet the university as well as the government have been divided. C. P. Snow has suggested the thesis of Two Cultures, existing side by side, but not in full communication with each other. This may be an oversimplification, and an exaggeration of the difference between science and the humanities. But each one of us knows and can observe within our own life that the thesis has some merit.

Science as Universal Truth: Science by nature is universal, and universal truth. The truth is not a national monoply of ours, the Russians, the Germans, or others. Science has built upon the discoveries of all, whether you start with the mathematical system of the Persians, the astronomy of the Middle East, the rational philosophy of the Greeks, or the engineering of the Romans. The fact is that every generation of all mankind builds upon all prior accomplishments, to the extent man knows that which has gone before.

Politics, on the other hand, is the art of gaining and using power. It has tended to be parochial and provincial, or becoming, in our century, national. The conflict between the nationalist pressures of politics and the internationalist thrust of science has especially plagued our generation. Our resolution of the conflicts has been a series of partial concessions on every side. Politics and "national interest" have demanded of both scientist and bureaucrat the use of security regulations, classification, or other restraint in publication, in an earnest effort to hide within that restraint our glimpses of the truth. Science has fought a partial and troubled rear-guard action against such pressures.

During the past generation the major powers, through political machinery, have made the concessions much sweeter by wooing science with every financial inducement. Funds have been available for rocketry, for aeronautics, and for astronautics. The challenges are: "Let's

go to the moon! Let's explore outer space! Let's bore a hole through the earth's mantle! Let's examine the cell and the secret of life! Let's build an airplane which will go three times the speed of sound!" Do not question the price; let the scientist set the price; the politician will provide the funds. Research and development have been favored with untold billions, every year, recently.

Indeed, in preparing the U.S. reply to the UN on the economic and social consequences of disarmament, we gave serious thought to the problem of transforming scientific resources to civilian uses when the heavy military bias of present scientific research has been taken away. For we have not asked the scientist what did he think was the most important thing to do. We have offered him funds if he would set his mind to answering parts of the question that the politician has chosen, and those funds have been generous in the extreme.

The Scientist as Soldier: Governments have long been among the patrons of science. Governmental checkbooks have been ample for those items which, at any given moment, have been most popular. In the past quarter century, the international competition in defense machines, weapons technology, and logistic support has demanded and commanded the time and talents of an increased number of the scientific community. The scientist's role has been heavily dictated by the conception of the national interest. The scientist has been asked to serve as an intellectual sleuth, to be secretive, to outthink each possible adversary, to develop every possible defense against every possible attack of the opponent.

But we face a changing awareness of what is the national interest as we go into the last one-third of the 20th century. For we now recognize, with respect to the major powers, the existence of a nuclear stalemate.

The Scientist as Peacekeeper: In recent years, there has been a growing concern of the risk to all humanity of further proliferation of nuclear knowledge. Dr. Davidon's paper, "The N*th* Country Problem" years ago made clear that a dozen more nations could afford to buy into the nuclear club; and, as you know, the French have about made it. One or two other nations may soon be admitted to the club. Under these circumstances, the risks of proliferation, the risks of escalation of a small dispute, and the risks of war by accident, all threaten genocide not just of one class or tribe, but of humanity itself. As President Johnson so effectively stated, "There are no victors in war, no losers in peace."

But all of this poses new problems for scientists. About the time that Congressmen Kastenmeier and I cosponsored the National Peace Agency Bill in 1960, Senator Kennedy cosponsored a similar bill for an Arms Control and Disarmament Agency. A year later the Kennedy proposal, under Kastenmeier's sponsorship, became law. Scientists are now asked to think about fail-safe systems, about inspection, detection, and related peacekeeping matters. Scientists as well as administrators are being forced to think in new categories.

Some years ago at Pugwash, scientists from both sides of the Iron Curtain undertook discussions which have continued almost every year thereafter. Such discussions are needed now more than ever to restore that essential minimum of confidence and trust in each other, without which no successful negotiation is possible. We need to be holding such conversations among scientists, journalists, citizens, and politicians, in order that we correctly understand each other. For, the alternative of misunderstanding threatens our survival itself. Fortunately, scientists are now working diligently at solving peacekeeping questions, both in the home countries and as advisers to the delegations to the UN 18-nation Disarmament Committee now in session at Geneva.

The Scientist as a Diplomat: With every passing year, the interdependence of man becomes more evident. It was out of a deep sense of history that President Kennedy chose July 4, 1962,

(*Continued on page 143A*)

Solution by simulation

HOW TO SIMULATE A "WAY-OUT" PROBLEM ON A DESIGNER'S DESK

A simplified method of studying models of airborne systems (whether STOL, SST or anything between) is being used successfully by aerospace engineering groups. Small scale analog computers, compact enough to fit on a desk top, have simulated the various control and propulsion elements of a growing list of complex airborne devices. They have also provided answers to problems in stress and vibration analysis and heat-transfer studies.

Having "built" a computer model of the system under test, the designer can make it react in a manner similar to the dynamic behavior of the real system in flight. Analog simulation permits dynamic analysis under varying operating conditions—applied simply by changing coefficient potentiometer settings.

Balloons in Aerospace

Even balloon-borne telescopes enter the aerospace realm, as in the design study made at Perkin-Elmer Corporation for Stratoscope II. The system is lifted by balloon to 80,000 feet, a level free of atmospheric turbulence. Stratoscope II provides photographic and spectrometric coverage of planets and galaxies. The star-tracking guidance system is designed to yield 0.02 arc—second tracking accuracy.

The pointing servos, simulated on EAI desktop analog computers, are highly non-linear, having a sensor with a linear zone equal to 1/600 of its operative range and a total d-c loop gain of over 3,000,000. According to Perkin-Elmer engineers the computers were extremely reliable and "their predicted servo response correlated very closely with measured performance in the actual system."

Perkin-Elmer designers also simulated the astronomer's manual radio-control, coarse pointing system for the telescope and the optical sensors of the guidance system. By using simulation, the engineers obtained considerable data which confirmed the design concept.

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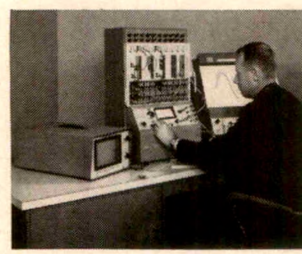

A free computer operations course is provided with every TR-20 and TR-48 purchase at one of the company's computation centers. In addition, EAI offers tuition courses in areas throughout the country (send for details and date of next course).

Every new owner of an EAI computer automatically becomes a participant in the growing EAI Applications Library, and is eligible to receive simulation studies pertaining to all categories of research and design.

Send for detailed literature on EAI TR-20 and TR-48 analog computers, or arrange for a problem-solving demonstration in your plant or office—at no charge.

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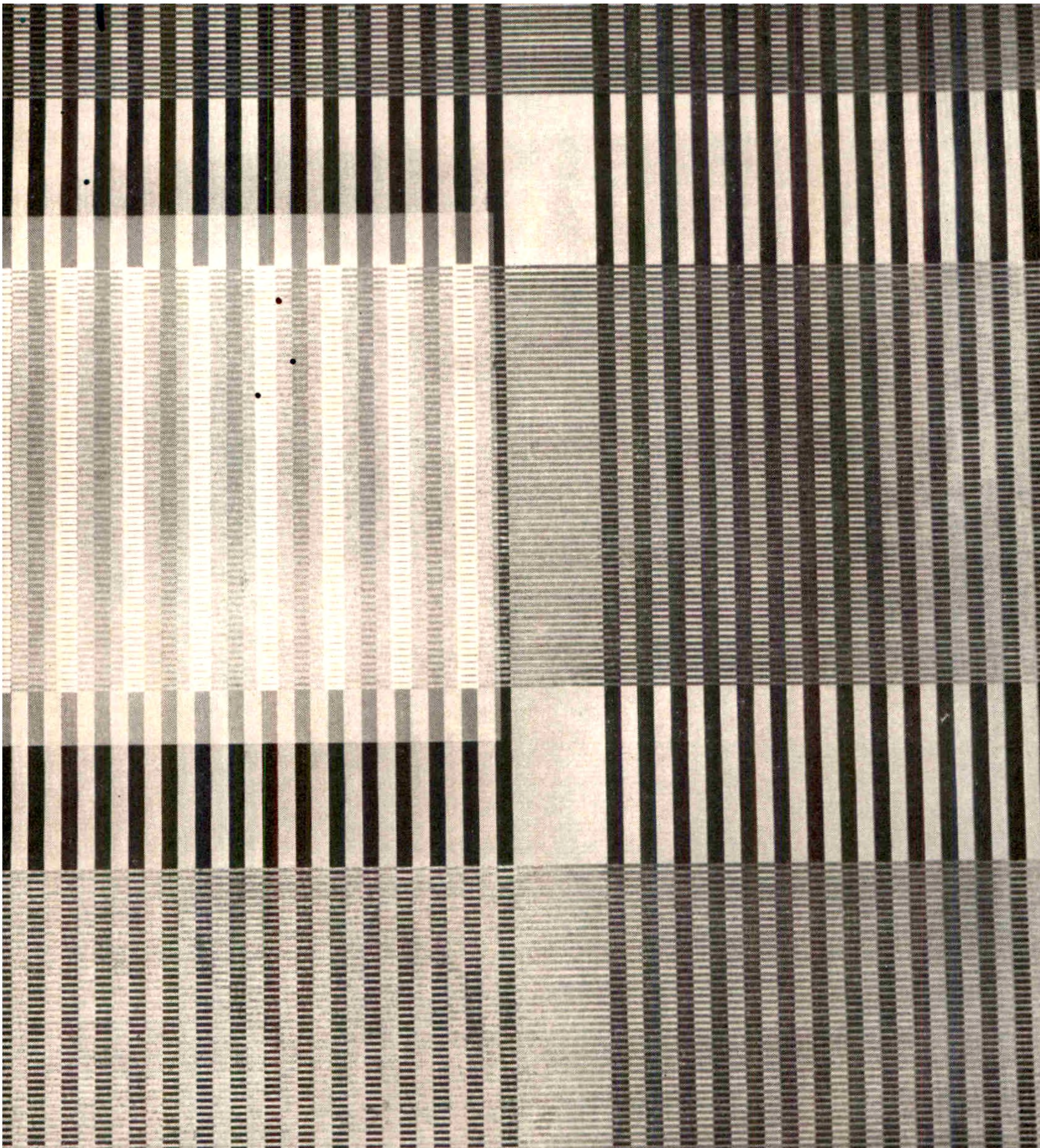
ELECTRONIC ASSOCIATES, INC., *Long Branch, New Jersey*

ADVANCED SYSTEMS ANALYSIS AND COMPUTATION SERVICES/ANALOG COMPUTERS/HYBRID ANALOG-DIGITAL COMPUTATION EQUIPMENT/SIMULATION SYSTEMS/SCIENTIFIC AND LABORATORY INSTRUMENTS/INDUSTRIAL PROCESS CONTROL SYSTEMS/PHOTOGRAMMETRIC EQUIPMENT/RANGE INSTRUMENTATION SYSTEMS/TEST AND CHECK-OUT SYSTEMS/MILITARY AND INDUSTRIAL RESEARCH AND DEVELOPMENT SERVICES/FIELD ENGINEERING AND EQUIPMENT MAINTENANCE SERVICES.



hin Film Memory Unit

Mark No 1 Wanting

(*Continued from page 140A*)

as the time, and the steps of Independence Hall in Philadelphia as the place, not to re-read the "Declaration of Independence," but to read a new Declaration of Interdependence. More rapid communication, greater travel, greater income, and a growing number of centers of scientific activity, all work to help make clear that there are many more opportunities and a much greater necessity for international scientific cooperation.

The U.S. belongs to a growing number of international agencies. Each year there are more formal international conferences where scientists must serve as advisers, or as representatives of their governments. Such conferences concern global matters of meteorology and aeronautics, of oceanography and atomic energy, of health and of food supply. There are more and more technical professional and scientific meetings within each of the various regions of the world. The UN Regional Economic Commissions for Asia and the Far East, Latin America, Europe, and Africa each involve more and more scientists in diplomatic roles.

Scientists are increasingly sought as advisers to both the bureaucracy and the legislative bodies. A deep sense of concern in the White House led to the creation of an office of Science Adviser to the President. There is also a Science Adviser to the Secretary of State. Some of my colleagues who served as junior members of Congressional committees which annually authorize billions of dollars for scientific research were keenly aware of their need for competent but disinterested scientific advice. There are no physical scientists, apart from medical doctors, elected to the Congress. Perhaps it is a good idea for some state to consider electing a competent scientist to public office. Since public policy requires a capacity for informed judgment in scientific matters, perhaps a few scientists might serve in elective office.

In any case, those in elective office must seek out the best scientific advice they can get. They welcome the expression of those few leaders in the scientific community who dare to comment on issues of public policy that lie within their own areas of competence. For the risk is that the only advice which administrators and legislators will normally get will come from scientists whose academic and research connections may give them a vested interest so strong that their advice may tend to reflect their personal welfare rather than promote the general welfare. The scientific community should keep this question high on its agenda.

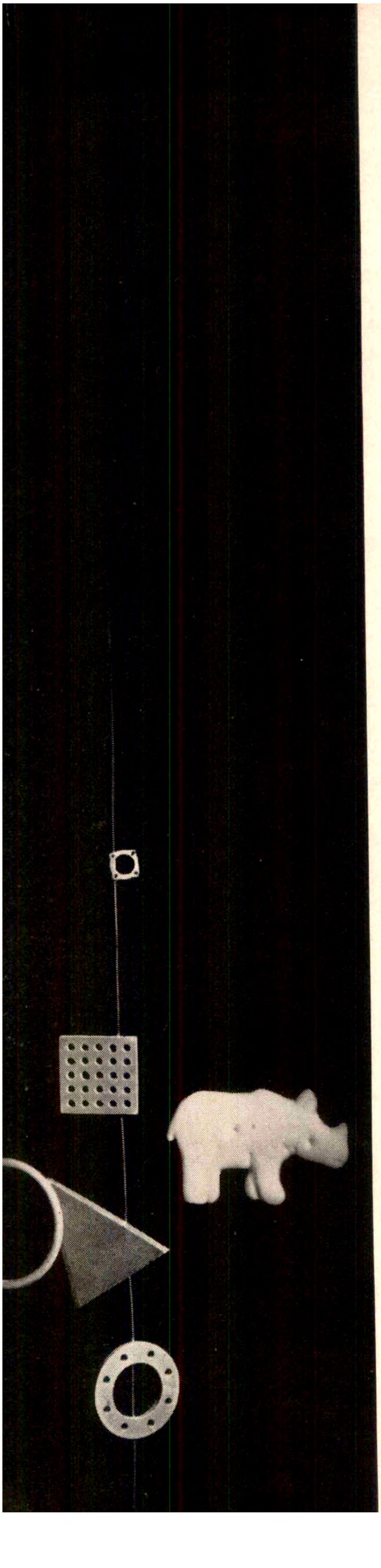
The Scientist as International Reporter: The Office of International Scientific Affairs in the Department of State, now headed by Dr. Ragnar Rollefson, distinguished professor of physics at the University of Wisconsin, employs scientists in at least 17 overseas posts, and the office anticipates establishing more such posts. The role of international scientific reporter is a two-way role. Information is being fed from these countries to the U.S.; such persons also provide an additional channel of information from the U.S. scientific community into these countries.

As a simple illustration of its possible significance, let us consider the report of Dr. H. N. L. Chinn, a deputy scientific attaché at the American Embassy in Bonn, Germany. He submitted the first comprehensive scientific report to our government as to the side effects of Thalidomide, December 22, 1961. This information, while slow in getting to her, was helpful to Dr. Kelsey's efforts. Now, steps are being taken to see to it that the information received from the field promptly reaches the desks in the U.S. where it will be most understood and useful.

There may be a task of recruiting persons who are both sufficiently competent specialists to command respect abroad, and yet are generalists enough to serve as reporters across the whole scientific spectrum. I trust that Colorado State University is now training young scientists who can see not only the disease on the leaf on the tree, yet also comprehend the whole forest as well.

The Scientist as International Saviour:

In the years following World War II there has been a growing zeal for development. During the war years the



Institute for Inter-American Affairs under Nelson Rockefeller made significant contributions in the fields of health and education in Latin America. Death rates have fallen sharply, which leads to the misnamed "population explosion." Technical co-operation programs were launched not only by the U.S., but by the UN, and increasingly by various other countries. Now, even little Israel is sending scientists and technicians into other countries to help in their development.

What these programs need, of course, are scientists who can serve overseas as diplomats, administrators, demon-strator-salesmen, teachers, bureaucrats, inventors, explorers, adventurers, linguists, and handymen. I trust that the students in classes at the universities are not only learning science but also these many other arts and skills, that they may serve more effectively inter-nationally.

The scientist can serve as an inter-national servant though he remains his whole life in his own classroom, or his own laboratory. For he has foreign students who will be in his classroom and in his laboratory and as he inculcates the centrality of integrity to these foreign visitors, he performs a vital task. As he excites their minds with the vision of the future, he becomes a missionary of progress. But he is a missionary who need not leave home. He need not be Ulysses to know, as Tennyson tells it:

> "Yet all experience is an arch where-through gleams that untraveled world whose margin fades forever and forever when I move."

And he must teach the importance of changes. If, in the U.S., we find ourselves doing some procedure the same way we did it five years ago, we stop and re-examine the situation. For we are convinced that surely there is a better way by now. Unhappily, the problem of development is a problem of working within societies that remain hostile to change, that believe that old ways are necessarily the best ways, that new ways are threatening.

The scientist must pursue truth with a singleness of purpose, for the truth will make us free. Yet the scientist must also be a full participant in humanity. The day must come when we disprove the thesis of two cultures by a blending of the two cultures. The scientists must understand and support the humanities and the social sciences as well, and join them in taking a completely human view of human beings. This must in-creasingly become the role of the scientist in international affairs.

BOOKS RECEIVED FOR REVIEW

TO THE MEMBERSHIP: *Our readers are reminded that this section of their journal is intended to be an information service listing first editions of the most important new scientific hardbound and paperbound books received for review in our Princeton Editorial Offices. Titles are for late 1963 and 1964 publications unless otherwise noted.*

From Academic Press:

Micromanipulators & Micromanipulation by H. M. EL-BADRY; 334 pages; $15.

Advances in Inorganic Chemistry & Radiochemistry, Vol. 5, edited by H. J. EMELEUS & A. G. SHARPE; 429 pages; $14.50.

Advances in Physical Organic Chemistry, Vol. 2, edited by V. GOLD; 288 pages; $10; 63s; London & New York.

The Transfer of Calcium & Strontium, edited by R. H. WASSERMAN; 443 pages; $11.50.

Fundamental Topics in Relativistic Fluid Mechanics & Magnetohydrodynamics, edited by ROBERT WASSERMAN & C. P. WELLS; 241 pages; $8.50.

Generic Names of Orchids, Their Origin & Meaning by R. E. SCHULTES & A. S. PEASE; 331 pages; $12.

Craigie's Neuroanatomy of the Rat, revised by W. ZEMAN and J. R. M. INNES; 230 pages; $8.50.

Non-Stoichiometric Compounds, edited by L. MANDELCORN; 674 pages; $22.50.

Non-Linear Wave Propagation with Applications to Physics & Magnetohydrodynamics by A. BEFFREY & T. TANIUTI; 369 pages; $12.

Pulmonary Deposition & Retention of Inhaled Aerosols by T. F. HATCH & P. GROSS; 192 pages; $5.95 cloth; $3.45 paper.

Metabolic Inhibitors, A Comprehensive Treatise, edited by R. M. HOCHSTER & J. H. QUASTEL; $24. to subscribers.

The Monosaccharides by J. STANĚK *et al.;* 1006 pages; $32.

Primary Processes in Photosynthesis by M. D. KAMEN; 183 pages; $5.50.

The Physiology & Biochemistry of Herbicides, edited by L. J. AUDUS; 555 pages; $16.50; London.

X-Ray Optics & X-Ray Microanalysis, edited by H. H. PATTEE, *et al.;* 622 pages; $22.

Radiation, Radioactivity, & Insects by R. D. O'BRIEN & L. S. WOLFE; 211 pages; $5.95 cloth; $3.45 paper.

A Photographic Atlas of Shark Anatomy by C. GANS & T. S. PARSONS; 106 pages; $3.95 paper.

Advances in Child Development & Behavior, edited by L. P. LIPSITT & C. C. SPIKER; Vol. I; 387 pages; $12.

Non-Glycolytic Pathways of Metabolism of Glucose by S. HOLLMANN, translated from the German & revised by O. TOUSTER; 276 pages; $12.

Advances in Virus Research, Vol. 4, edited by K. M. SMITH & M. A. LAUFFER; 277 pages; $11.50.

Cellular Membranes in Development, edited by M. LOCKE; 382 pages; $12; (The Twenty-Second Symposium, The Society for the Study of Development Growth, Storrs, Connecticut, June 1963).

Advances in Metabolic Disorders, Vol. 1, edited by R. LEVINE & R. LUFT; 366 pages; $12.

Advances in Space Science & Technology, Vol. 5, edited by F. I. ORDWAY, III; 334 pages; $13.

Advances in Protein Chemistry, Vol. 18, edited by C. B. ANFINSEN, JR., *et al.;* 335 pages; $14.

Chemical Applications of Infrared Spectroscopy by C. N. R. RAO; 683 pages; $19.50.

Advances in Biological & Medical Physics, Vol. 9, edited by J. H. LAWRENCE & J. W. GOFMAN; 496 pages; $16.

The Formation of Wood in Forest Trees, edited by M. H. ZIMMERMANN; 562 pages; $16. (Symposium at Harvard Forest, Petersham, Mass.).

Evolutionary & Genetic Biology of Primates, Vol. II, edited by J. BUETTNER-JANUSCH; 330 pages; $12.50.

Discrete & Continuous Boundary Problems by F. V. ATKINSON; 570 pages; $16.50.

Energy Band Theory by J. CALLAWAY; 357 pages; $10.

Theory of Superconductivity by J. M. BLATT; 486 pages; $12.50.

Photophysiology, Vol. I: *General Principles; Action of Light on Plants;* 377 pages; $14; Vol. II: *Action of Light on Animals & Microorganisms; Photobiochemical Mechanisms; Bioluminescence,* edited by A. C. GIESE; 441 pages; $15.

Ionization in High-Temperature Gases, edited by K. E. SHULER & J. B. FENN; 409 pages; $5.75.

International Review of Experimental Pathology, Vol. 2, edited by G. W. RICHTER & M. A. EPSTEIN; 446 pages; $16.

(Continued on page 150A)

85 CPS TO 8000 CPS
AUDIO SPECTRUM ANALYZER

NEW! KAY Sona-Graph 6061-A

The 6061-A audio spectrum analyzer is an up-to-the minute solid state adaptation of our 661-A Sona-Graph. It provides the same permanent visual records of Amplitude vs Frequency, Amplitude vs Time, and Frequency vs Time vs Amplitude, BUT in less than ONE-THIRD THE PREVIOUS TIME and with increased clarity.

Plug-in Filter

In order to maintain continuity in interpreting audio records via the well known patterns of the Sona-Graph, the original filter bandpass characteristics have been maintained. In order to add new flexibility, the filter assembly has been made as a front panel plug-in unit. It can be interchanged with other filter plug-ins to give special displays, higher resolution, etc. Both the Amplitude Display and the Scale Magnifier units are also available as plug-in heads.

Variable AGC

A further step to provide flexibility in addition to established characteristics has been taken by providing a variable AGC compression control.

Single Compact Unit

The 6061-A is a compact, single unit, in plac of the previous three plus accessories. It new dimensions of 25 x 20 x 18½", includin accessories, makes installation much easier. A new modular construction using high-grad glass-epoxy printed circuit units makes a muc more rugged unit, fit for a more demandin environment. It also provides for real ease o maintenance. Components and test points ar more easily accessible, and complete sub-cir cuits can be replaced in minutes.

As on later models of the 661-A, the 4 inch drum is interchangeable with a 12 inch drum when a larger recorded display is desired Additional new features include selectable linear or logarithmic frequency scale expan sion and a 500 cps calibrating signal. Excellent stability and reliability is assured by the use of silicon transistors.

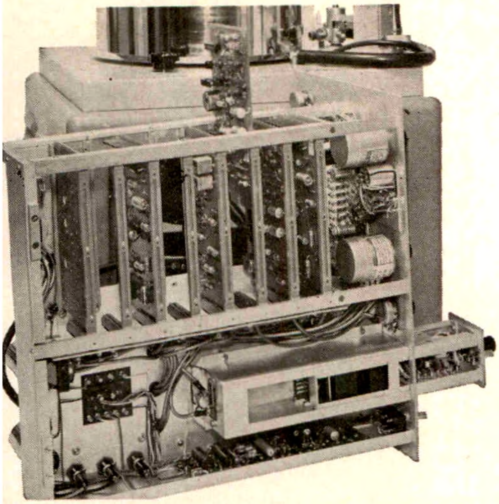
The Spectrograph

The Sona-Graph is a sound spectrograph which produces permanent, visual records of complex audio-frequency waves and provides three dif ferent recorded analyses of these waves. The first of these analyses (see Display No. 1) relates frequency and intensity to time; the second (see Display No. 2) relates intensity (over a wider dynamic range than the first) to frequency at any selected time; the third relates the average available amplitude t time.

Visual Records

The visual records which contain the analyses of the recorded waves are made on non-photo graphic, current-sensitive, facsimile-type paper. The paper is mounted on a drum whose

KAY ELECTRIC COMPANY
PINE BROOK, MORRIS COUNTY, N.J.



xis is the same as the turntable on whose eriphery the continuous magnetic recording ilm is deposited. This arrangement provides utomatic time synchronization. The record is raced by a stylus which advances upward and t the same time changes the apparent center requency of the analyzing band-pass filter. A high-frequency current applied to the stylus s varied in amplitude in proportion to the mount of energy passed by the bandpass ilter. The first type of record obtainable (see Display No. 1) displays time on the abscissa, requency on the ordinate, and intensity as hading between gray and black. The second ype of record (see Display No. 2) displays inensity in db vs. frequency at as many as six elected times. This display has a dynamic range in intensity of 35 db. The third type of record (see Display No. 3) displays average available amplitude vs. time within a dynamic range of 34 db maximum. These visible speech records can be read and phonetic studies made with these displays.

SPECIFICATIONS

Frequency Range: 85 to 8000 cps.

Response: ± 2 db over entire range.

Resolution: 45 and 300 cps.

Recording Time: Any selected 2.4 second interval of any audio signal within frequency range.

Recording Medium: Magnetic tape.

Aspect Ratio of Sonagram: 4″ by 12″ with 4 inch drum; 4″ by 37.5″ with 12 inch drum

Record-Reproduce Amplifier Characteristics Flat, or approx. 15 db High-frequency pre-emphasis for voice studies.

Microphone: Altec-Lansing 633A dynamic

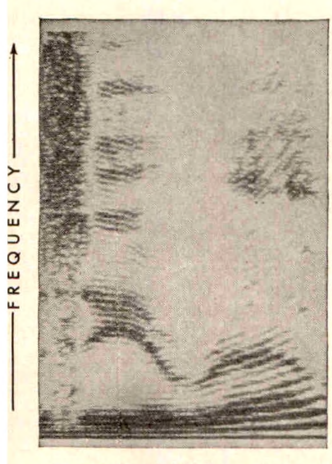
Input Impedance: 50 ohms, low level; 600 ohms and 10K, high level.

Power Supply: Input approx. 100 watts 117 volts (± 10%) 60 cps (50 cps operation on request), Power Supply electronically regulated.

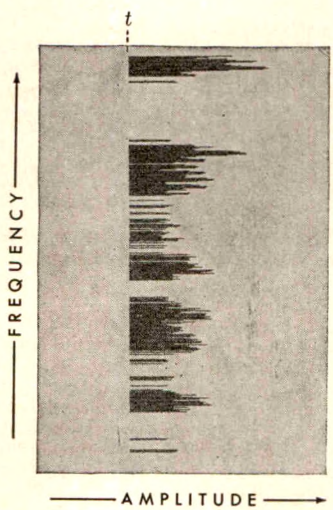
Dimensions: 25″ h by 20″ w by 18½″ d.

Weight: 95 lbs.

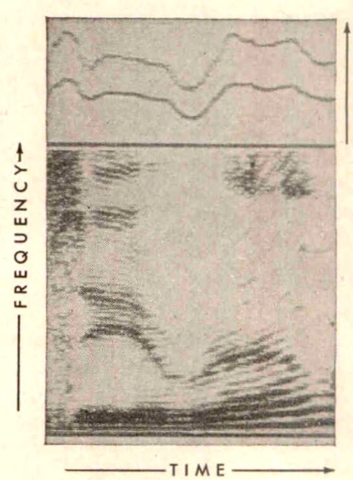
Accessories Supplied: 250 sheets Sonagram recording paper, Type B; Sectioner Micrometer Plate No. 668-A.



DISPLAY #1



DISPLAY #2



DISPLAY #3

ELECTRIC CO PA Y

PINE BROOK, MORRIS COUNTY, N.J.

(*Continued from page 146A*)

Methods in Carbohydrate Chemistry, Vol. 4, edited by R. L. WHISTLER; 335 pages; $13.50.

Advances in Lipid Research, Vol. 1, edited by R. PAOLETTI & D. KRITCHEVSKY; 418 pages; $14.

Lipid Pharmacology, Vol. 2 of *Medicinal Chemistry,* edited by R. PAOLETTI; 538 pages; $17.50.

From **Addison-Wesley:**

Elementary Biophysics, Selected Topics by H. T. EPSTEIN; 122 pages; $1.75; 1963.

Linear Algebra by P. C. SHIELDS; 288 pages; $7.50.

Introduction to Functional Analysis by B. Z. VULIKH; 404 pages; $10; 1963.

College Calculus with Analytic Geometry by M. H. PROTTER & B. MORREY, JR.; 897 pages; $11.50.

Elements of Mechanical Analysis by C. R. MISCHKE; 386 pages; $9.75; 1963.

Algebraic Numbers by S. LANG; 163 pages; $7.

Sequential Machines: Selected Papers, edited by E. F. MOORE; 266 pages; $7.50.

Nuclear Chemistry & Its Applications by M. HAISSINSKY, translated from the French by D. G. TUCK; 834 pages; $22.50.

From **Aldine Publishing Co.**

Mounds of the Near East by S. LLOYD; 120 pages; 30 plates; no price given.

Monsoon Lands of Asia by R. R. RAWSON; 256 pages; $6.

Sturgeon Hooks of Eurasia by G. de ROHAN-CSERMAK; 155 pages; $6.

A Hundred Years of Metallurgy by *W. H.* DENNIS; 342 pages; $8.95.

Physical Chemistry by A. J. MEE with J. C. SPEAKMAN; 719 pages; $5.95.

Introduction to Atomic & Nuclear Physics by D. E. CARO *et al.;* 222 pages; $6.

From **Basic Books:**

Introducing Science by A. ISAACS; 244 pages; $4.95.

Beyond Time by M. SIFFRE; 228 pages; $4.95.

100 Problems in Elementary Mathematics by H. STEINHAUS; 174 pages; $4.95.

Life Beyond the Earth by V. A. FIRSOFF; 320 pages; $7.50.

From **Doubleday & Co.:**

Science Study Series Anchor Paperbound Originals: *Waves & Beaches* by W. BASCOM; 267 pages; $1.45; *Faraday, Maxwell, & Kelvin* by D. K. C. MACDONALD; 143 pages; $1.25; *Rutherford & the Nature of the Atom* by E. N. DA C. ANDRADE; 218 pages; $1.25.

American Museum Science Books, Natural History Press: *A Short History of Biology* by I. ASIMOV; 183 pages; $1.25 paper; also hardbound.

Africa & Africans by P. BOHANNAN; 260 pages; $1.25 paper; also hardbound.

Photographing Nature by D. LINTON; 262 pages; $1.95 paper.

Invitation to Anthropology by D. L. OLIVER; 102 pages; $0.95.

Charles Darwin by G. DE BEER; 290 pages; $4.95.

From **Gordon & Breach:**

Quantum Field Theory & the Many-Body Problem (Introductory Lectures) by T. D. SCHULTZ; 150 pages; $6.95 cloth; $3.95. Vol. 1 in the Series.

Telemetry (Monographs on Rockets & Missiles) by R. E. YOUNG; 78 pages; $3.95.

Point Defects in Metals by A. C. DAMASK & G. J. DIENES; 314 pages; $19.50; Professional edition, $9.50.

From **Harper & Row:**

Cognition—Theory, Research, Promise, edited by C. SCHEERER; 226 pages; $4. (Papers from the Martin Scheerer Memorial Meetings, University of Kansas, May 1962).

The Oculomotor System, edited by M. B. BENDER; 556 pages; $16. (Hoeber Medical Division).

Statistics, An Introductory Analysis by T. YAMANE; 734 pages; $8.75.

Reference Groups (Exploration into Adolescent Conformity & Deviation) by M. SHERIF & C. M. SHERIF; 370 pages; $6.

Science: The Glorious Entertainment by J. BARZUN; 322 pages; $6.

A Chemical Background to Nursing & Other Paramedical Programs by G. F. GRILLOT; 398 pages; $5.75.

From **The Macmillan Co. including Pergamon Press & The Free Press of Glencoe:**

Technical Dictionary of Radio & Telecommunication Installations (English, German, French, Russian) by H. PLOHN, *et al.;* 1001 pages; $30.

Biochemistry & Physiology of Plant Immunity by B. A. RUBIN & YE. V. ARTSIKHOVSKAYA; 358 pages; $14.

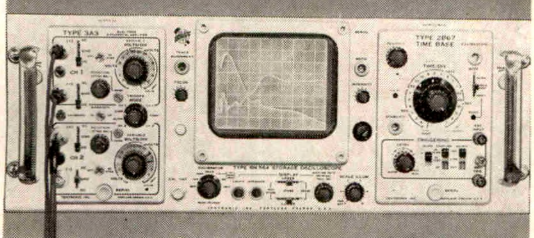
Foundations of Electrical Engineering: Fields-Networks-Waves by K. SIMONYI, translated by D. JONES & A. SEKEY; 848 pages; $15.

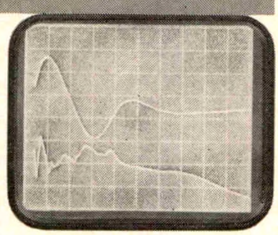
An Introduction to Mathematical Analysis by R. A. RANKIN; 607 pages; $11.50.

Biological Effects of Deuterium by J. F. THOMSON; 133 pages; $7.

From Low-Speed Aerodynamics to Astronautics by T. VON KARMAN; 82 pages; $3.75.

extra value in this Tektronix Type RM564 storage oscilloscope





Display shows ability of the Type RM564 to store low-level, single-shot events. Upper trace shows displacement of a leaf spring due to a single shock. Output of a strain-gage bridge (fastened to the spring) was fed to Channel 1 of the Type 3A3 operating in the differential mode, at 100 microvolt/cm sensitivity. Lower trace shows acceleration imparted to a leaf spring under test. Output from an accelerometer was fed to Channel 2 of the Type 3A3 at 2 millivolt/cm sensitivity. The sweep speed of traces was 20 milliseconds/cm.

Type RM564 Storage Oscilloscope (without plug-ins)	$1035
Type 3A3 Dual-Trace Differential Amplifier Unit	790
Type 2B67 Time-Base Unit	175

U.S. Sales Prices f.o.b. Beaverton, Oregon

here's why:

■ saves space

The Type RM564 occupies only 7 inches of standard rack height, yet has a full 8-cm by 10-cm display area.

■ provides more flexibility

The Type RM564 accepts combinations of 17 amplifier and time-base plug-in units for such applications as multi-trace, low-level differential, sampling, others—including matched X-Y displays using the same type amplifier units in both channels.

■ adds new convenience

The Type RM564 presents stored displays as well as conventional displays. **And these stored displays can be presented on the full screen or on the upper or lower half of the split screen crt—with conventional displays on the other half.** Storage time can be over one hour, with erase time less than one-fourth second. This new convenience for stored displays permits detailed waveform analysis and simplified waveform comparisons, in many instances, without resorting to photography.

Of course, conventional displays can be displayed on the full screen when the Type RM564 is not used for stored applications.

storage features:

■ split-screen storage . . . full-screen storage . . . writing-speed enhancement . . . repetitive-trace integrater . . . trace-position locater . . . remote-erase facility.

other features:

■ edge-lighted graticule . . . controllable illumination . . . 3.5-kv accelerating potential . . . z-axis input . . . amplitude calibrator . . . electronically-regulated power supplies . . . operation from 105 v to 125 v, or 210 v to 250 v, 50-to-60 cps.

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***Tektronix U. K. Ltd.**, Harpenden, Herts*

Histochemistry & Cytochemistry (Proceedings of 1st International Congress), edited by R. WEGMANN; 481 pages; $15.

Trace Analysis of Semiconductor Materials by J. P. CALI; 282 pages; $10.

Solid Lubricants & Surfaces by E. R. BRAITHWAITE; 286 pages; $12.

Problems in the Design & Development of 750 MW Turbo-Generators by V. P. ANEMPODISTOV, *et al.;* 76 pages; $4.50.

Recent Trends in the Development of the Theory of Plasticity by W. OLSZAK, *et al.;* 196 pages; $7.50.

The Application of Computing Technique to Automatic Control Systems in Metallurgical Plants by A. B. CHELYUSTKIN; 225 pages; $10.

An Introductory Course of Mathematics & Theoretical Mechanics in 4 Vols., Vol. III: *Theoretical Mechanics* by C. PLUMPTON & W. A. TOMKYS; 370 pages; $4.

Progress in the Chemistry of Fats & Other Lipids, Vol. VII, Part I, edited by R. T. HOLMAN; 164 pages; $5.75 paper.

Azeotropy & Polyazeotropy by W. SWIETOSLAWSKI; 226 pages; $10.

Propagation of Radio Waves at Frequencies Below 300 kc/s, edited by W. T. BLACKBAND; 478 pages; $20.

The Bending & Stretching of Plates by E. H. MANSFIELD; 148 pages; $6.50.

Theory & Structure of Complex Compounds, edited by B. JEZOWSKA-TRZEBIA-TOWSKA; 707 pages; $17.50; Symposium, Poland, June 1962.

Complexing & Hydrothermal Ore Deposition by H. C. HELGESON; 128 pages; $8.50.

Inorganic Ultramicroanalysis by I. ALIMARIN & N. PETRIKOVA; 151 pages; $6.

Asynchronized Synchronous Machines by M. M. BOTVINNIK; 75 pages; $5.

Equations of the Mixed Type by A. V. BITSADZE; 160 pages; $8.50.

Accelerators of Charged Particles by B. S. RATNER; 120 pages; $3.50.

Effects of Ionizing Radiation on the Reproductive System, edited by W. D. CARLSON & F. X. GASSNER; 478 pages; $14. Proceedings of Colorado State University International Symposium.

High Sensitivity Counting Techniques by D. E. WATT & D. RAMSDEN; 348 pages; $12.

Telemechanics by V. S. MALOV; 100 pages; $5.

Short Guide to Geo-Botanical Surveying by S. V. VIKTOROV, *et al.;* 158 pages; $9.

Classical Thermodynamics of Nonelectrolyte Solutions by H. C. VAN NESS; 166 pages; $6.

Chemistry in Premixed Flames by C. P. FENIMORE; 119 pages; $5. (Vol. V of *The International Encyclopedia of Physical Chemistry & Chemical Physics*).

Radiation Medicine, edited by A. I. BURNAZYAN & A. V. LEBEDINSKII; 363 pages; $12. (Vol. of *International Series of Monographs on Nuclear Energy, Biology Division.*)

Vistas in Botany (Vol. III *Recent Researches in Plant Physiology*), edited by W. B. TURRILL; 316 pages; $15.

Topological Methods in the Theory of Nonlinear Integral Equations by M. A. KRASNOSEL'SKII; 395; $10.

Biochemical Lesions & Lethal Synthesis by R. PETERS; 321 pages; $10.

Progress in Nuclear Physics, Vol. IX, edited by O. R. FRISCH; 310 pages; $15.

Peptides (Proceedings of 5th European Symposium, Oxford, September 1962), edited by C. T. YOUNG; 269 pages; $12.50.

Progress in Biophysics & Molecular Biology, Vol. XIII, edited by J. A. V. BUTLER, *et al.;* 328 pages; $12.75.

Scientist (Opportunities for Careers) by R. S. MORISON; 207 pages; $3.95; Macmillan.

Fatigue Resistance by P. YE. KRAVCHENKO; 122 pages; $5; Macmillan.

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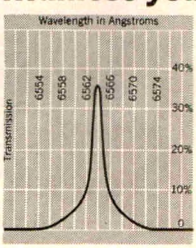

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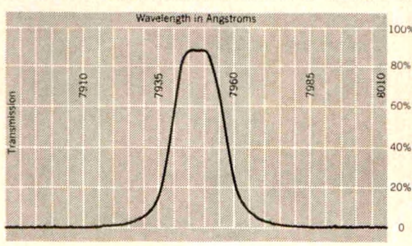

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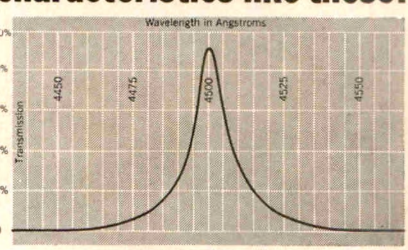

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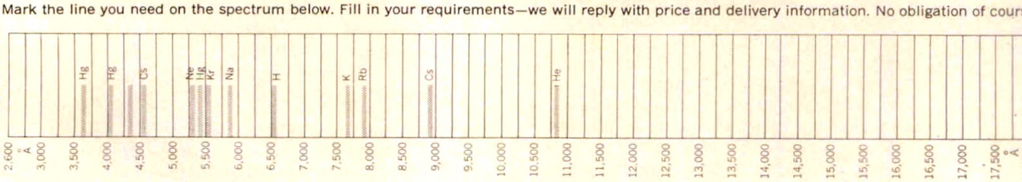

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Bandirantes & Pioneers by V. MOOG; 316 pages; $6.95.
Focus & Diversions by L. L. WHYTE; 235 pages; $5.

From **Basic Books:**
Ancient & Medieval Science (from the Beginnings to 1450), edited by R.

TATON; 551 pages; $17.50. Vol. I, *A History of Science;* A. J. POMERANS translator.

From **International Atomic Energy Agency** International Publications, Inc., 317 E. 34th St., New York 16, sole U.S. distributors). Paper only.

Radioisotope Applications in Industry, A Survey; 131 pages; $2.50.

Radiation Damage in Reactor Materials; 762 pages; $15. Final Proceedings of Venice Symposium May 1962.

Operating Experience with Power Reactors, Vol. I; 526 pages; $10; *Vol. II;* 412 pages; $8.50; Vienna Conference, June 1963.

Heavy Water Lattices: 2nd Panel Report: Vienna Conference, Feb., 1963; 647 pages; $13.

From **American Gas Assoc., Inc.** (605 3rd Ave., New York 16, N. Y.):

Kinetics & Mechanism of Active Sulfur Formation and Corrosion of Copper by Active Sulfur by R. G. CALDWELL; 136 pages; $3.50 paper;

From **Gamut Press:**

The Psychology of Laughter, A Study in Social Adaptation by R. PIDDINGTON; 224 pages; $4.50 cloth; $1.95 paper.

From **Canadian Peace Research Institute,** Clarkson, Ontario:

In Your Opinion by J. PAUL & J. LAULICHT (Leaders' & Voters' Attitudes on Defense & Disarmament); 140 pages; $2.95 paper.

From **The National Association for Mental Health, Inc.,** 10 Columbus Circle, New York 19, N. Y.:

Directory of Resources for Mentally Ill Children in the U.S.; 96 pages; $2.00 paper.

From **Taplinger Publishing Co.:**

Hydrodynamic Superposability by R. BALLABH; 45 pages; $3.75 paper.

From **W. B. Saunders Co.:**

Atomic Energy Encyclopedia in the Life Sciences, edited by C. W. SHILLING; 474 pages; $10.50.

From **Yearbook Medical Publishers, Inc.** (35 E. Wacker Dr., Chicago 1, Ill.):

Radiation Effects in Physics, Chemistry & Biology, edited by M. EBERT & A. HOWARD; 510 pages; $15. (Proceedings of 2nd International Congress of Radiation Research, Harrogate, Britain, August 1962); 1963.

From **Chilton Books:**

Men of Space, Vol. 6 by S. THOMAS; 286 pages; $5.95.

America's New Policy Makers (The Scientists' Rise to Power) by D. W. COX: 298 pages; $6.95.

From **Philosophical Library:**

The Senses of Animals by L. H. MATTHEWS & M. KNIGHT; 240 pages; $7.50.

From **Holden-Day, Inc.:**

Interpretation of Mass Spectra of Organic Compounds by H. BUDZIKIEWICZ, *et al.;* 271 pages; $8.75.

From **International Physical Index, Inc.:**

Russian Physics Quarterly (A source book of Soviet experiment & theory), Vol. 1, No. 1; $100 per yr. from the *Index,* 1909 Park Ave., New York 35, N.Y.

From **Royal Society, Burlington House,** London, W. I: *Biographical Memoirs of Fellows, Vol. 9;* 321 pages; $6.

Journal of Applied Probability, J. GANI, Editor-in-Chief (Dept. of Statistics, The Australian National University, Box 4, Canberra, Australia); 2 half-yearly numbers annually from May 1964; $8 yearly for individuals.

From **Doubleday & Co.** (Natural History Press):

For the American Museum of Natural History: *Biology of Birds* by W. E. LANYON; 175 pages; $3.95 hardbound; *Exploration of the Moon* by F. M. BRANLEY; 127 pages; $3.50 hardbound; both available paperbound.

From **Ginn & Co.** including Blaidell Publishing Co.:

Interpolation & Approximation by P. J. DAVIS; 393 pages; no price given.

From **Harper & Row:**

Self-Renewal, The Individual & the Innovative Society by J. W. GARDNER; 141 pages; $3.50.

In a more or less experimental effort, a list of individuals, with whom National Headquarters had lost contact when the Post Office returned mail marked "Not Found" or "Unknown," was published in the March issue of AMERICAN SCIENTIST. This initial listing contained 161 names. Response to the appeal of National Headquarters has been most gratifying and information has been received for 50 of those listed.

Each year more than 25,000 address changes are made for the members of The Society of the Sigma Xi. One-third of the membership has an address change each year. Immediately following the publication of each issue of AMERICAN SCIENTIST and the mailing of each communication to the Chapter-at-Large, National Headquarters receives a multitude of notices concerning address changes and among these are always a number of notifications of "Address Unknown." Of course notification of address change, by necessity, must be primarily the responsibility of the individual concerned. National Headquarters, however, will make every effort not only to maintain the addresses reported but to obtain those which it has failed to receive through normal channels. In this effort, the following report is made:

FOUND

(Numbers refer to listing on page 50A of the March 1964 issue of the AMERICAN SCIENTIST)

No. 3.	No. 42.	No. 122.
5.	43.	123.
17.	44.	125.
23.	45.	128.
26.		129.
31.	46.	136.
34.	49.	110.
36.	50.	111.
37.	53.	113.
38.	60.	114.
40.	66.	118.
69.	87.	1.45
70.	90.	1.46
75.	95.	1.54
77.	96.	1.56
81.	102.	1.57
83.	105.	1.59

LOST

If anyone can assist by furnishing information as to the present location of these lost members, the Society would be most appreciative.

162. Stephen Otis Atherton
163. Bhavani Belavady
164. Samuel H. Black
165. Eric MacDonald Erskine
166. Richard W. Fish
167. William S. Ginell
168. Robert William Hansen
169. Robert G. Heltsley
170. Charles P. Howard
171. George Jaffe
172. John W. James
173. Douglas Harvey Johnson
174. Marion Johnson
175. Thyril L. Ladd
176. Fred K. Manasse
177. Richard L. McArthy
178. Theodore Morse
179. Paul E. O'Neil, Jr.
180. Mrs. Felix Oppenheim
181. Alan Owen Pittet
182. Warren J. Rabourn
183. Ogden B. Ramsay
184. Robert G. Sandberg
185. Robert L. Schoenberger
186. Ronald S. Stone
187. Thomas E. Sweeney
188. Joseph R. Wagner
189. Barbara Woody

(Continued on page 170A)

Notice of the correct address should be sent to the Office of the Executive Secretary, The Society of the Sigma Xi, 51 Prospect Street, New Haven, Connecticut 06511.

"The human race is so emotional . . ."

The year was 1939, and newly-named Chief of Staff George Catlett Marshall faced a mammoth task: preparing for war a country which thought itself insulated from the fires of Europe and Asia. "The human race," he remarked later, "is so emotional that good common sense seldom prevails in a great crisis." Fortunate it was for America and her allies that the good common sense of General Marshall prevailed, as he first built history's greatest military machine and then led it to victory.

But the soldier's responsibility did not end with one victory. Appointed post-war Secretary of State, he gave war-shattered Western European nations a second one . . . over poverty, hunger, and the communism which might have engulfed many of them but for his European Recovery Program. That is what *he* called it; most of us knew only of the "Marshall Plan."

By whatever name, it won him the Nobel Prize, and rightly. What tribute is equal to a man who brings unity, integrity, hope, and deliverance to nations at war *and* to a world at peace?

In the national interest, the Research Analysis Corporation applies scientific thought to major military, technical, economic, and political problems. Non-profit and multidisciplinary, RAC invites scientists and engineers with advanced degrees and superior capabilities to investigate its career opportunities; please send your resume to Mr. John G. Burke, Professional Staffing, Research Analysis Corporation, McLean, Virginia 22101 (suburb of Washington, D. C.). An equal opportunity employer.

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RESEARCH ANALYSIS CORPORATION

Important Notice. The attention of all readers and publishers is called to the present address of the Editorial Office of AMERICAN SCIENTIST

33 Witherspoon St., Princeton, N. J. ,08540

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If your name or address has changed from that on file at headquarters at Sigma Xi or RESA, please return the form below to:

Office of the Executive Secretary
51 Prospect Street
New Haven 11, Connecticut 06511

This is to notify you of the following change of address or name. (Please print or typewrite.)

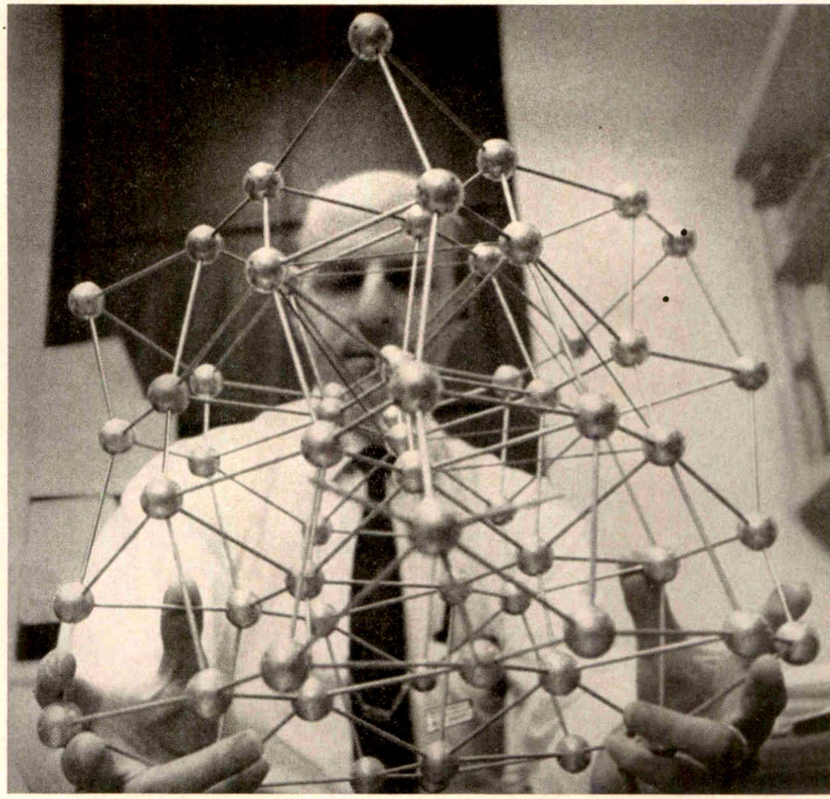
Former: Name..
Street..
City..................................Zone..............
State..
Zip #............

Present: Name..
Street..
City..................................Zone..............
State..
Zip #............

Affiliation: (Please give name of Chapter, Club or Branch)

☐ Member-at-Large
☐ ....Chapter
☐ ....Club
☐ ....RESA branch
☐ Subscriber

Last Issue Received: ....



190. Edward D. Amstutz
191. Keith Maxwell Anderson
192. Miriam Magdalene Ausman
193. John McDonell Augustine
194. Mirza Nazir.Baig
195. Achmad Baiquni
196. Miguel D. Barandiarian
197. Patricia May Barger
198. Robert Barker
199. Tery L. Barr
200. Kenneth R. Barton
201. Robert John Bell
202. William H. Beeartz
203. Eugene P. Brantly
204. Barry Breindell
205. Edward N. Brody
206. Linda Jane Brody
207. John Brayton Bronzan
208. Robert Andrew Brown
209. E. A. Bartkus
210. Michael James Cambern
211. Ricardo Antonio Carreras
212. George Cheryan
213. David Kuo-Kien Chow
214. Thomas W. Clarkson
215. Gerrard Coetzee
216. Roy Herbert Cook
217. Dr. Rebecca S. Cox
218. Kathleen Joy Cramer
219. Joseph Colcord Curtis
220. Eugene S. De Haven
221. Gerald E. Dreifke
222. Robert J. Dummel
223. John Campbell Edgar, Jr.
224. Dean A. Eggert
225. Floyd Elroy Ellertson
226. William Clinton Elsik
227. John A. Enright
228. W. Buell Evans
229. Michael B. Fallgatter
230. Warren William Fee
231. Donald Linn Felt
232. Peter Sutcliffe Fernald
233. Linda Richardson Foster
234. Yhushi Fukmoto
235. Stanley Joseph Gamble
236. John Kenneth Garland
237. Edward R. Garrett
238. Anil C. Ghose
239. David A. Gilbart
240. Gary Gitnick
241. Burdette M. Glancey
242. Robert Douglas Graham
243. Marie Church Griffin
244. Gerald Groden
245. William L. Hagan
246. Roger D. Haight
247. Max Reutger Hasche
248. Douglass Frederick Hayman, Jr.
249. Dr. Edith Haynes
250. Thomas E. Hoffer
251. Ryukichi Honda
252. Werner K. Honig
253. Dale Kay Huggins
254. Lynn David Ikenberry
255. Robert Lynn Ingalls
256. Kent E. Johnson
257. Thomas Lee Keith
258. Charles Lindley Kemp
259. Robert B. Kerr
260. Fred J. Kieras
261. Jeremy Kisch
262. Joseph K. Kovach
263. Mutsuo Koyama
264. Raymond M. Kramer
265. Eleanor Svea Kutlich
266. John Peter Lekner
267. Marius Lepage
268. Paolo Lepri
269. Stanley J. Leuty
270. Lt. John S. Lew
271. Arnold A. Liebman
272. Joel Fred Lubar
273. John Leo Madden
274. Robert F. McCracken USN
275. Charles W. McDougal
276. Michael B. McElroy
277. Dr. Manchery P. Menon
278. Roger Edwin Messick
279. Wayne Doyle Mathews
280. Carlos B. Metzadour
281. George W. Miskimen
282. David Campbell Morrison
283. Stanley W. Morse
294. Mrs. Effat A. Mourad
295. Thomas Fabian Necheles
296. Augustine N. Njoku-Obi
297. Donald C. Noble
298. Robert M. Oman
299. Diarmuid Padraig O'Mathuna
300. Capt. Carl A. Ostrom
301. Sara Leah Page
302. Dr. Edward D. Palmes
303. Robert John Parfitt
304. Gerald Earl Peabody
305. Gary R. Pearson

THE SOCIETY OF THE SIGMA XI

STATEMENT BY THE TREASURER

The Executive Committee, in March 1963, voted that certain changes be made in the format and method of preparing the Financial Statements of the Society. The purpose of these changes was two-fold: to give fuller disclosure and to set forth more precisely the true financial picture of the Society.

At the March 1964 meeting of the Executive Committee, the Treasurer was requested to recast, in the new format, the Financial Report for 1963 which appeared on pages 157–160 of the March 1964 issue of AMERICAN SCIENTIST, and to prepare it for publication in the June 1964 issue of AMERICAN SCIENTIST.

This was deemed advisable so that there may be a comparison with 1963 operations when the 1964 Financial Statements, which will be in the new form, are published in March 1965.

Although the total figures are consistent between the two presentations of the 1963 Financial Statements, that which follows differs mainly in the use of *total* figures rather than *net*, the establishment of a Reserve for Deferred Payment of Honoraria for Lecturers, the accounting for Bills Payable, and the realignment of certain classifications of income and expense.

HARVEY A. NEVILLE
Treasurer

REVISED REPORT OF THE TREASURER FOR 1963

BALANCE SHEET AS OF DECEMBER 31, 1963 WITH 1962 FIGURES FOR COMPARISON

		1963	1962
ASSETS			
Bank Balances:			
Union & New Haven Trust Co. (Checking Account)			
January 1, 1963	$ 66,049.65		
Cash Receipts, 1963	434,427.67		
Cash Disbursements, 1963	376,914.98		
Transferred to Trust Dept.	51,226.60	$ 72,335.74	$ 66,049.65
Connecticut Savings Bank		10,000.00	10,000.00
New Haven Savings Bank			
January 1, 1963	5,208.31		
1963 Increase from Investment Account	789.61	5,997.92	5,208.31
First Federal Savings & Loan Assn.		10,000.00	10,000.00
National Savings Bank		10,000.00	10,000.00
National Savings Bank (Berg Fund)		3,000.00	3,000.00
Society for Savings (Hartford)		10,000.00	10,000.00
Investments:			
January 1, 1963	227,397.86		
Securities Purchased	50,436.99		
Securities Donated	6,172.50	284,007.35	227,397.86
Total Assets		$405,341.01	$341,655.82

Liabilities

		1963	1962
Accounts Payable			
Dec. 31 Bill for Dec. Issue			
American Scientist	$ 27,314.34	$ 27,314.34	...
Semi-Centennial Fund		15,050.00	15,050.00
Berg Memorial Fund		3,000.00	3,000.00
William Procter Fund		22,624.55	22,624.55
Ada P. McCormick Fund			
January 1, 1963	2,697.33		
Honorarium Paid Out, 1963	250.00		
Interest Received, 1963	122.00	2,569.33	2,697.33
The Hsien Wu and Daisy Yen Wu Fund			
January 1, 1963	10,157.50		
Securities Received, 1963	2,310.00		
Securities Received, 1963	3,862.50	16,330.00	10,157.50
Anonymous Fund	2,500.00	2,500.00	...
Reserve for Grants-in-Aid of Research			
January 1, 1963	74,717.49		
Net Decrease, 1963	9,251.42	65,466.07	74,717.49
Reserve for Prepaid Assessments			
January 1, 1963	13,516.00		
Less 1963 Assessments	6,888.75		
Plus Assessments Prepaid in 1963	5,955.00	12,582.25	13,516.00
Reserve for Life Members			
January 1, 1963	100.00		
Fees Received, 1963	7,350.00	7,450.00	100.00
Reserve for Deferred Payment of Honoraria for Lecturers	6,600.00	6,600.00	...
Reserve for Royalties to Authors			
January 1, 1963	1,434.83		
Royalties Received, 1963	86.77	1,521.60	1,434.83
Securities Reserve		377.37	377.37
Capital Reserve			
January 1, 1963	180,000.00		
Plus Distributed Gain, Dec. 1963	30,000.00	210,000.00	180,000.00
Surplus			
Balance, January 1, 1963	17,980.75		
Plus Undistributed Gain, 1963	21,289.09		
Less Adjustment for Accounts Payable, Dec. 1963	27,314.34	11,955.50	17,980.75
Total Liabilities		$405,341.01	$341,655.82

1963

Cash Receipts

	(Actual)		(Budget)
Annual Assessments			
Chapters	$147,990.00		$159,000.00
Clubs	14,260.50		15,000.00
Membership-at-Large	73,625.80		66,000.00
Fees			
Life Membership	7,320.00		...
Promotions	2,367.44		1,000.00
Initiations	41,984.00		40,000.00
Installations and Charters			
Chapters	200.00		200.00
Clubs	175.00		250.00
Contributions			
James Cattell Fund	3,000.00		3,000.00
Membership-at-Large	27,615.60		
Chapters and Clubs	4,067.93	34,183.55	
Anonymous Donor	2,500.00		35,000.00
Subscriptions to American Scientist			
Non-members	9,466.22		10,500.00
Advertising	63,654.30		80,000.00
Science in Progress			
Sales	98.15		500.00
Royalties	86.77		
Lectureships			
Fees	6,750.00		7,200.00
Insignia Royalties	2,619.02		2,400.00
Investments			
Interest and Dividends	13,744.80		13,750.00
RESA			
American Scientist	8,800.00		8,800.00
Lectureships	2,000.00		2,000.00
Grants-in-Aid of Research	2,000.00		2,000.00
Miscellaneous	102.14		100.00
Total Cash Receipts	$434,427.67		$446,700.00

1963

Cash Disbursements

	(Actual)	(Budget)
Salaries		
Headquarters	$ 54,399.76	$ 50,000.00
American Scientist	10,600.00	13,000.00
Science in Progress	500.00	750.00
Social Security	1,979.63	1,400.00
Benefits	367.35	300.00
Honoraria		
American Scientist	2,225.00	1,500.00
Lectureships	12,270.00	16,000.00
Rent		
Headquarters	2,448.00	2,500.00
American Scientist	1,440.00	1,440.00
Printing		
American Scientist	127,617.46	150,000.00
Manual of Procedure	1,053.96	1,000.00
Initiate Booklet	1,902.08	1,400.00
Charters	342.50	225.00
Certificates	7,857.42	6,000.00
Other	1,509.40	4,100.00
Postage and Mailing		
Headquarters	7,194.50	4,600.00
American Scientist	11,585.64	5,000.00
Science in Progress	83.69	
Supplies and Expenses		
Headquarters	6,649.09	2,000.00
American Scientist	1,980.75	3,000.00
Equipment	865.16	1,000.00
Travel and Expenses		
Officers and Committees	5,929.05	10,500.00
Lecturers	6,110.73	10,000.00
Research—Grants-in-Aid	82,279.95	84,000.00
Advertising Expense	17,583.76	20,000.00
Management Survey	6,757.00	7,500.00
Miscellaneous		
Science Service	300.00	300.00
E.D.P. (Rent & Service)	2,099.14	1,920.00
Bank Charges	17.24	100.00
Other Misc.	966.72	
Total Cash Disbursements	$376,914.98	$399,535.00

1963

Total Receipts (and Distribution of Gain)

Total Receipts:		
Cash	$434,427.67	
Securities—Gift of Mrs. Daisy Yen Wu	6,172.50	
Total Receipts		$440,600.17
Total Disbursements		376,914.98
1963 Total Gain		$ 63,685.19

Distribution of Gain (Adjustment of Funds and Reserves)	
The Hsien Wu and Daisy Yen Wu Fund	$ 6,172.50
McCormick Fund	
Increase: $122 Interest	
Decrease: $250 Honorarium	(128.00)
Reserve for Grants-in-Aid of Research	
Decrease: $9,251.42	(9,251.42)
Reserve for Prepayment of Assessments	
Increase: $5,955.00 Prepaid in 1963	
Decrease: $6,888.75 Prepaid for 1963	(933.75)
Reserve for Life Membership	
Increase: $7,350.00	7,350.00
Reserve for Deferred Payment of Honoraria for Lecturers	
Increase: $6,600.00	6,600.00
Anonymous Fund	
Increase: $2,500.00	2,500.00
Reserve for Authors' Royalties	
Increase: $86.77	86.77
Capital Reserve	
Increase: $30,000.00	30,000.00
Total Distributed Gain	$ 42,396.10
Total Undistributed Gain	21,289.09
	$ 63,685.19

COMMENTS BY THE TREASURER OF SIGMA XI

Frequently, the Officers of the Society and members of the Executive Committee are asked: How much does the Society spend on Grants-in-Aid of Research? What does it cost to publish American Scientist? How are the costs shared in the National Lectureship Program?

There are a number of methods by which these values might be obtained—but none actually gives a complete picture for there are a number of irreducibles or unallocatable costs. However, the following figures reflect a reasonable comparison of the costs and income for these activities:

From the preceding Financial Statement for 1963—

Activity	Expenses	Income
American Scientist		
Salaries	$ 10,600.00	
Honoraria	2,225.00	
Rent	1,440.00	
Printing	127,617.46	
Postage & Mailing	11,585.64	
Supplies & Expense	1,980.75	
Advertising Expense	17,583.76	
		$63,654.30 Advertising
		9,466.22 Subscriptions (non-members)
		8,800.00 RESA
Allocation of National Headquarters Expenses for American Scientist*		
Salaries	$ 15,000.00	
Supplies	1,500.00	
Total Expense	$189,532.61	$81,900.52 *Total Income*
Grants-in-aid of Research		
Grants	$ 82,279.95	
Travel & Expense	500.00	
Clerical	3,000.00	
		$34,183.55 Contributions
		2,000.00 RESA
		3,000.00 Cattell Fund
Total Expense	$ 85,779.95	$39,183.55 *Total Income*
Lectureships		
Honoraria	$ 12,270.00	
Travel & Expense	6,110.73	
Clerical	3,480.00	
		$ 6,750.00 Fees
		2,000.00 RESA
Total Expense	$ 21,860.73	$ 8,750.00 *Total Income*

(* Allocation of National Headquarters Expenses—Salaries and Supplies—in support of American Scientist—are *estimates*)

THOMAS T. HOLME

Professor of Industrial Engineering, Yale University, was elected at the Sixty-fourth Annual Convention of Sigma Xi in Cleveland, Ohio, in December 1963, to a third Five-year term as Executive Secretary of the Society. He succeeded Professor George A. Baitsell of Yale University as Acting Secretary in 1953.

AMERICAN SCIENTIST, *52*, 1964

AMERICAN SCIENTIST

SUMMER · JUNE 1964

ATOM TO ADAM

By MELVIN CALVIN and G. J. CALVIN

THE course of the social history of man from the time he became capable of recording his progress is popularly considered the only "recorded" history. This "day" in the history of mankind is so brief in relation to all history and has been so exaggerated in importance as to obscure the long course of evolutionary development preceding this period. Because man has emphasized his own personal history, much as an individual views the importance of his own brief years in relation to recorded history, the natural laws which govern the development of man—and the countless life forms which exist with him—are frequently isolated from those laws which govern other matter in the universe.

It is difficult to consider living things as a far product on the long continuum from (organic) element to Einstein. However, as we learn ever more details of the composition of living things, the course becomes clearer, and the experimental evidence more corroborative, that the entities known as "living" follow the simple molecular laws of chemistry and physics, just as do the chemicals on the shelf. It becomes clear, too, that atoms can be combined into molecules and macromolecules in test tubes today in much the same way as was possible under the conditions when the earth was new.

The expanded knowledge about the atomic and molecular constituents of which living things are made, together with an increased understanding of the way molecules interact with each other, i.e., communicate with each other, so as to produce what we now recognize as living organisms, has had two very interesting results. The first has been to stimulate scientists to create hypothetical schemes leading from the primeval nonliving earth to the present day [1, 3, 10, 11, 12, 13, 14, 16, 25, 27, 28, 29]. The second has been to induce scientists to devise experimental ways to test some of these schemes in points at which they might be amenable to experimental laboratory tests. A certain degree of success in a variety of these laboratory experiments has, in turn, modified the original theories and has even led to new experiments in both chemistry and biology.

Terrestrial Chemical Evolution

Conjecture as to the origin of life on the earth must involve knowledge of the behavior of molecules in the prebiotic period as well as a detailed and intimate understanding of the composition and function of living matter. The complexity of the problem is both simplified and exaggerated by contemplation of the qualities which distinguish nonbiotic systems from those we call "alive." There is a high level of disagreement among scientists who try to define the minimum requirements for living systems. This fact is in itself significant for it demonstrates that the borderline between the living and the nonliving is a difficult thing to recognize. There is no problem in distinguishing the living from the nonliving at the extremes of the scale; there is difficulty only at the borderline.

At this borderline, a living system has no sharply defined characteristic, easily distinguishing it from a nonliving system. Rather, a living system is a molecular aggregate possessing a collection of properties which make it indisputably recognizable as "living" at only one end of the scale and as "nonliving" at the other end of the scale. But, somewhere in between, the nature of this collection of properties is such that there are those who will say that the system is "alive" and those who will say it is not.

Of these various properties, I am going to choose two which I think everyone will accept as necessary, although perhaps not sufficient, attributes of a molecular system in order for it to be called "alive." These two properties are (1) the ability of such a molecular aggregate to transfer and transform energy in a directed way; and (2) its ability to remember how to do this, once having "learned" it, and to transfer, or communicate, that information to another system like itself which it can construct. The two are, restated: (1) The transfer and transformation of *energy* and (2) the transformation and communication of *information*. In a sense the second—that is, information transfer—may be thought of as including the energy transfer problem as well, but I like to think of them as separate problems.

Molecular Construction: There seems to be a fairly general agreement that the primitive earth was originally surrounded by an atmosphere which was composed primarily of reducing material—that is, the atoms of hydrogen, oxygen, carbon, and nitrogen in their fully reduced, or hydrogenated, state. This corresponds to the relative cosmic abundance of the very same elements—hydrogen being the most abundant [20], oxygen the next, etc. Thus, the atmosphere of the primitive earth is envisioned as containing mostly the atoms of hydrogen, carbon, nitrogen and oxygen combined only with the overwhelmingly dominant hydrogen giving molecular hydrogen, methane, ammonia, and water.

What kinds of compounds can we make from these primordial molecules? Ultimately we will have to make from these starting materials the

chief components of living organisms which are three polymers that we recognize as essential, namely, the proteins, the nucleic acids, and the polymeric substances known as polysaccharides, cellulose, starch, etc. (composed of simple sugars made of carbon, hydrogen, and oxygen with relatively small amounts of nitrogen and a few other elements (Fig. 1)). We have tried to devise ways and means of making the monomeric materials of which these polymers are constructed and then of finding ways of evolving the polymers themselves by nonbiological routes. It is at this level that we can inject experimental observation, and this has been done not only in our laboratory but elsewhere as well.

We thus have to accomplish two stages of chemical evolution, i.e., (1) we have to transform the primeval molecules made of carbon, oxygen, and nitrogen, attached to hydrogen, into the small primitive molecules which are the monomers from which (2) the polymers are eventually evolved.

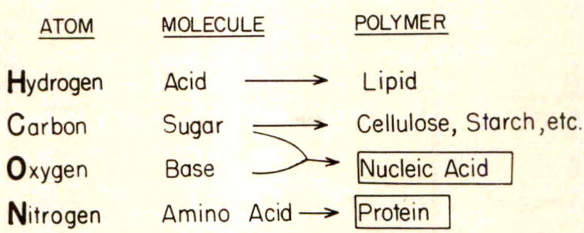


Fig. 1. Schematic representation in chemical terms of the set of formations which have to be accomplished from the atoms to produce the structure of the cell.

The time scale which is available to perform these transformations is given in Figure 2. The formation of the present earth took place somewhere around 4.7 billion years ago. Overlapping with this period begins the period of chemical evolution which covers almost the entire time scale. The earliest known generally accepted fossils are less than one billion years old. However, it has been reported that there is organic matter—formed elements and even recognizable structures—in formations about 2 billion years old in the Gunflint chert of Northern Michigan [45]. This chert is a carbonaceous formation in which one can, in section, see formed elements which appear to be primitive blue-green algae. The earliest known fossils in an unequivocal sense appeared in the Cambrian period, but I believe that the primitive blue-green algae formations in the Precambrian material from Michigan might push the dating of the early fossils back about another billion years. While the period of chemical evolution may be shorter than shown in Figure 2, organic evolution, as it is commonly defined, must have begun at least 2 billion years ago. The moment that living organisms appear, the processes which we de-

scribe as nonliving or chemical (evolution) may have had a rather sharp decline because the living material would rapidly absorb and convert the primitive molecules and the relatively slow nonbiological chemical change would be cut off.

You will notice from Figure 2 that the evolution of mammals is relatively recent, and the evolution of man himself by the process of random mutation and selection occupies an even still shorter period of the time scale. What I have called "Social Evolution" is so small that it can't be

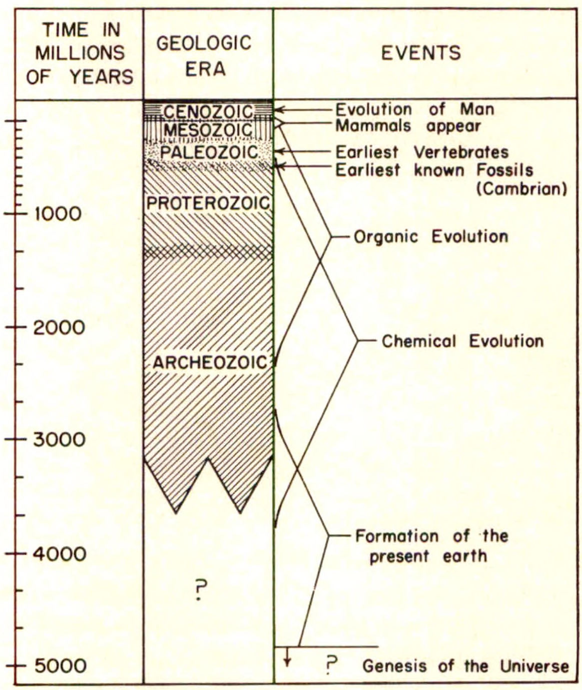


Fig. 2. Time scale for total evolution.

represented on this time scale; in fact, it is a matter of only a few thousand years. One might say a new kind of social evolution has only just begun in the last century or two, since man has had in his own hands the ability to manipulate a living organism in a directed way.

We will concentrate on the period of chemical evolution and the borderline period of biological evolution, during which living cells first appeared. Photosynthesis must also have begun at this time [4], and, as soon as this phenomenon appeared, the whole scheme of animal evolution and plant evolution, as we now see it in the fossil record, began and really "exploded" at an enormous rate.

I am not going to be concerned too much with this intermediate region

of organic evolution except to describe its principles of direction which were determined (and still are) by the principles of chemical evolution which gave rise to the living organisms in the first place.

Figure 3 depicts the primeval and the primitive organic molecules with which chemical evolution began about 4.7 billion years ago. The energy sources that were used in the transformation were any of several: ultraviolet light from the sun, cosmic radiation, radioactive minerals on the surface of the earth, and the streaming of the atmosphere due to thermal convection giving rise to the generation of electrostatic potentials and electric discharges. These various sources of energy induced the fracturing of the carbon-hydrogen, hydrogen-oxygen, hydrogen-nitrogen, and hydrogen–hydrogen bonds in the primeval atmosphere to give high en-

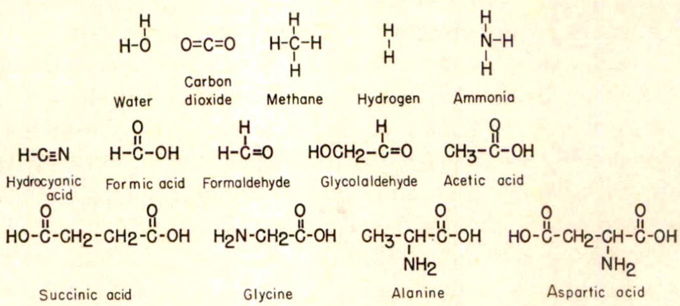


Fig. 3. Primeval and primitive organic molecules.

ergy intermediates which were then recombined to intermediately stable forms shown in the second row of Figure 3. In the last dozen or so years this kind of evolution has been demonstrated in the laboratory. In our first experiments in 1950 using ionizing radiation from an accelerator, we showed the conversion of carbon dioxide in water and hydrogen to produce formic acid, formaldehyde, etc. [15]. Within a couple of years after that, Stanley Miller used methane and ammonia in the reaction mixture with the resulting appearance of amino acids—glycine, alanine, aspartic acid [21, 22]. This started the search for all of the primitive monomeric molecules which are the constituents of the three polymers so essential for the construction of living organisms [30].

In general, these processes of energy transformation—ultraviolet light, radioactivity, electric discharge—involving the conversion of the primeval to primitive molecules—took place in a random way. The same forces which disrupt the primeval molecules can also disrupt the primitive monomeric molecules as well. One must therefore seek autocatalytic processes which would select among the various possible recombinations and which favor one or another of these primitive molecules [1]. In gen-

eral, this search has been successful. By adding mineral catalysts, for example, iron, zinc, etc. (which may give rise to more complex substances) to such reaction mixtures, porphyrins show up quite early in the evolutionary scheme and, in turn, these are catalytic for their own formation, thus giving rise to a molecular selection in the course of chemical evolution.

It is possible to produce from the primeval atmosphere a collection of primitive monomeric molecules in solution. It has recently been shown that HCN is formed in this way [32], and the pentamer of HCN, adenine, as well, even in this dilute solution [35]. From adenine (a nucleic acid constituent) it is possible to make other heterocyclic bases which are necessary for the construction of the nucleic acids. Not only adenine but sugars are also formed from the formaldehyde which comes directly from carbon dioxide, or from methane, hydrogen, and water. Thus, in this mixture there is already present the base and the sugar.

In the last several months, Ponnamperuma has obtained adenosine upon ultraviolet irradiation of a dilute solution of ribose [36] (the five carbon sugar which is required for the formation of the riboside), together with adenine. If this adenosine is irradiated with ultraviolet light absorbed by the adenine in an aqueous solution of a pyrophosphate ester, adenylic acid is obtained and even adenosine triphosphate, ATP, as well [37]. It has been demonstrated that not only can building blocks of today's organisms be generated by abiogenic processes, but the basic "energy currency" (ATP) used by all organisms can be formed in a similar abiogenic conversion of the prime energy sources, ionizing energy and light.

Polymerization: The whole sequence of events from methane to the mononucleotide and the other monomers has now been carried out by the random supply of energy of the right kind to the primeval molecules. Is it possible to construct, under similar circumstances, the polymers which are required both for structure and for information storage and transfer? The nucleotide is still not a polymer—it is only the monomeric unit which ultimately has to combine with another one through phosphate linkages. In order to get the polymer from, for example, adenylic acid, it will be necessary to do another condensation reaction between the phosphoric acid group of one molecule and one of the alcohols on another adenylic acid molecule; thus a bifunctional unit is maintained which can be used in further condensation, leading eventually to the useful polymer.

In the case of the amino acids, we also have a bifunctional form (the carboxyl at one end of the chain and the amino group at the other), and there are a variety of R groups, depending on the molecules with which one starts. These bifunctional molecules can then be combined into a polymeric form by a *dehydration* reaction. Figure 4 shows the nature of

the dehydration reaction of the precursors which lead to the proteins, polysaccharides and nucleic acids, the biopolymers. This is an area ripe for experimental exploration, although enough success has already been achieved to warrant their presumption.

The question now is: what kind of dehydrating agent(s) is (are) necessary to bring this sequence of events about in a nonbiological system

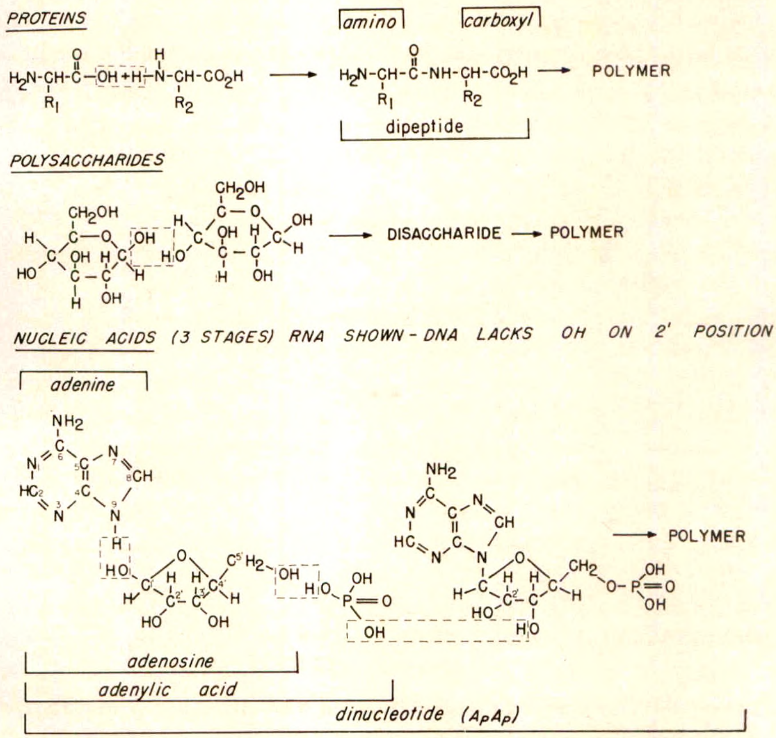


Fig. 4. Dehydration reactions leading to biopolymers.

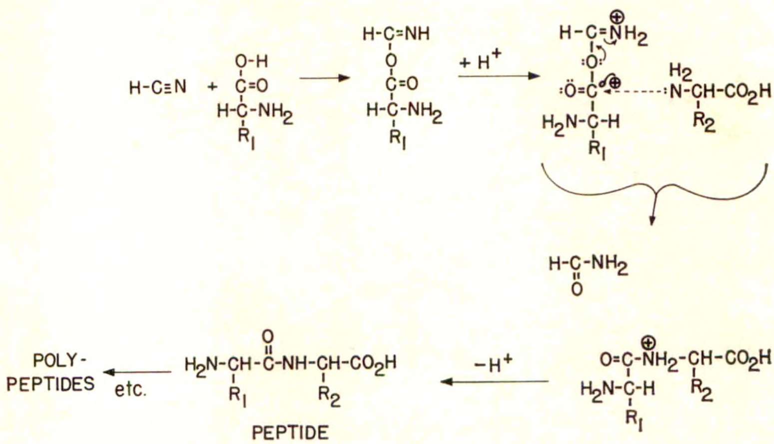
in a dilute water solution? This kind of thing was recently done in the laboratory by using hydrogen cyanide, HCN, itself as a dehydrating agent. HCN is an anhydride of formamide and it may behave as a specific dehydrating agent, even in dilute aqueous solution. By heating amino acids in solutions of HCN, one is able to obtain not only adenine but polymers of the amino acids as well [18]. Figure 5 shows a possible mechanism by which HCN might function as a specific dehydrating agent. The analogy of this reaction to the established synthetic reaction using carbodiimide is apparent [17]. The possible more or less specific dehydration condensation function of the wide variety of phosphoric anhydride deriva-

tives has long been under exploration [17], and their more recent [43, 49, 37] application in aqueous solutions is even more promising.

There are also other means of obtaining polypeptides, polyphosphates, esters, etc., for example, in a nonaqueous medium such as one might get in tidal pools by evaporation and concentration.

Generation of Order and New Information

If it is accepted that we can construct polypeptides, polynucleotides, and polysaccharides by nonbiological methods, this in itself is a major step toward the structured features which are required for organized



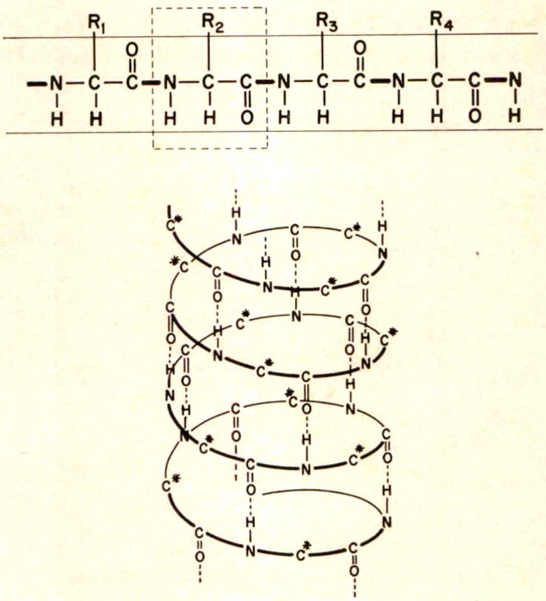
DEHYDRATION OF AMINO ACIDS TO FORM PEPTIDES BY HCN IN AQUEOUS AMMONIA

Fig. 5. Possible mechanism of peptide formation with HCN as dehydrating agent

energy conversion and information transfer. Evidence is accumulating that the secondary, tertiary, and even quaternary structure of proteins and nucleic acids are thermodynamically stable forms of a particular primary structure, resulting from the simple dehydration polymerization reactions. I would like to make some experimental points which will help demonstrate that such structural information which is required for both efficient energy conversion and for information transfer is contained ultimately in the monomeric sequences that one finds in either of these two principal types of polymers—namely, nucleic acids and the proteins.

Protein Structure and Function: From amino acids one can make a polypeptide of some particular amino acid sequence, and this polypeptide will assume a definite structural arrangement which is not random in solution.

The structure assumed depends upon the various atoms of which it is constructed, particularly on the amide carbonyl and the amide NH group, and upon an interaction between the R groups themselves. These latter may be any of a variety of types: hydrophobic bonds, van der Waals' interactions, electrostatic interactions, hydrogen bonds, etc. For our purpose it is enough to know that there are forces which hold the



PROTEIN STRUCTURE

Simple structural principles — Variety of chemical reactivity

Fig. 6. Protein structure.

polypeptides in definite conformations, such as shown in Figure 6. The polypeptide contains within its linear structure the necessary information to give rise to the well known helix. This helix of the protein is a secondary macrostructure of a higher degree of order than that defining the amino acid sequence alone and which is spontaneously assumed by the primary structure. Evidence for this is abundant.

For example, it is possible to destroy the secondary structure and then see if it will reform. This phenomenon is demonstrated in Figure 7, which shows it for polyglutamic acid. At pH 8, the gamma-carboxyl groups on the end of each glutamate are ionized to produce negative charges which repel each other strongly enough to destroy the alpha helix

structure. This is manifested in the form of the optical absorption of the amide linkage. When the amide linkages are randomly oriented with respect to each other (random coil at pH 8), there is a higher optical absorption. At pH 4.9, when the carboxyl groups are not ionized, the alpha helix is reformed and there is a new optical transition in the ordered array of the amide linkages. The effect is reversible [44]. This demonstrates that the secondary structure of the polymer is already contained in the primary amino acid sequence.

Much more than the limited information required for the secondary structure is contained in the primary amino acid sequence. The so-called

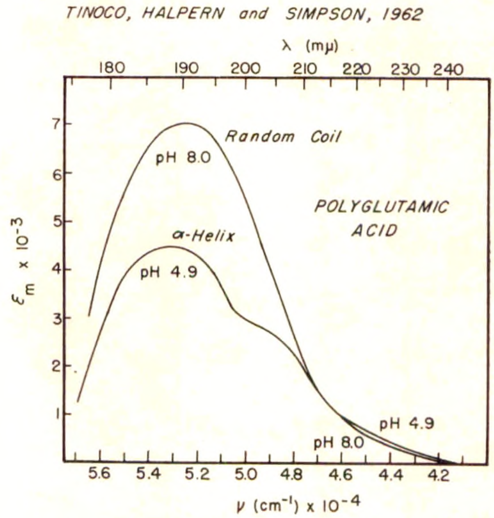


Fig. 7. Absorption spectrum of polyglutamic acid in both helical and random coil forms.

tertiary structure is contained as well. The tertiary structure may be considered as the folding of the alpha helix coil on itself in some special way. In general, the way in which tertiary structures are arranged with respect to each other could be called quaternary structure. The definition of this fourth level of order is at present under lively discussion by chemists, physicists, and biologists [8]. It appears that both tertiary and quaternary structure follow from primary sequence as well.

The evidence for the fact that the tertiary structure is contained in the primary amino acid sequence is just coming to hand. (It appears to have existed for some time, but has not been recognized as such.) The primary form of that evidence is the reversible denaturation of enzymes. Enzymes in general are proteins which not only require a particular amino acid sequence and a helical structure, but need helical sections structurally re-

lated in space to each other in the proper way. For example, it is not uncommon to have the functional groups of an enzyme consist of an imidazole group of a histidine residue and a hydroxyl group of a serine residue, and they may be in different parts of the protein chain. In the active form of the enzyme they function together, side by side, on the same substrate. Since we know the primary sequence, we therefore know that the helical part must have tertiary structure which brings the histidine and serine residues together so that the two groups can function cooperatively, for example, in the hydrolysis of an ester. Thus we know that there is tertiary folding.

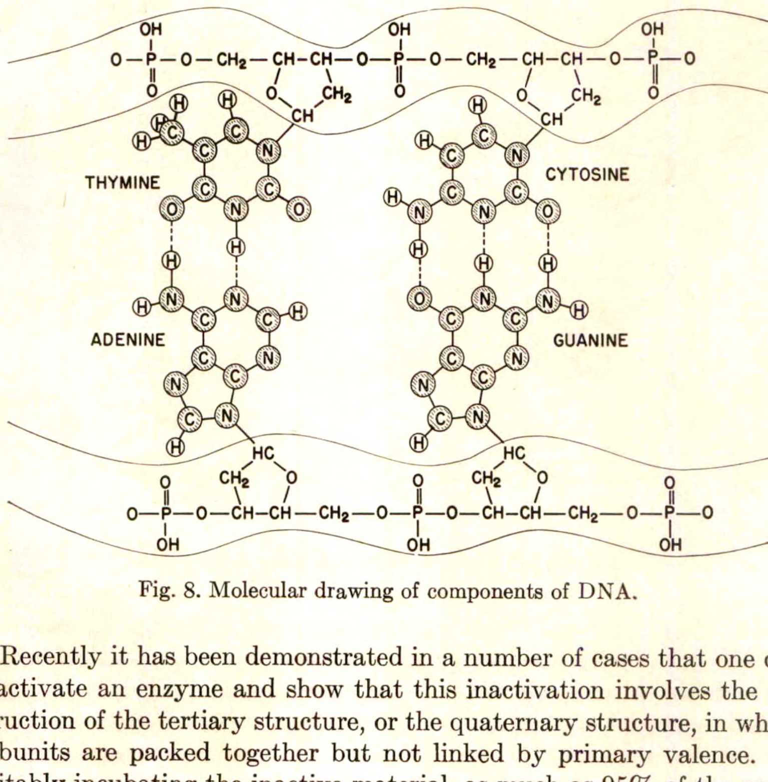


Fig. 8. Molecular drawing of components of DNA.

Recently it has been demonstrated in a number of cases that one can inactivate an enzyme and show that this inactivation involves the destruction of the tertiary structure, or the quaternary structure, in which subunits are packed together but not linked by primary valence. By suitably incubating the inactive material, as much as 95% of the enzymatic activity can be recovered. This means that the tertiary and quaternary structures (depending upon what the enzyme is) have been reformed spontaneously [8]. One can carry this denaturation clear down to the random coil level—that is, go all the way down to the primary structure—and can climb almost all the way back through the alpha helix into the tertiary folding and even into the quaternary aggregation. This last has indeed been achieved in the case of the enzyme aldolase [8].

The whole purpose of this discussion is to demonstrate that the pri-

mary sequence of the R groups in a polypeptide contains all of the necessary information—enough to construct a whole active-functioning structure as a thermodynamically stable form.

Nucleic Acid Structure and Function: The same phenomenon which was discussed for the structured arrangement in the polypeptide holds true for the polynucleotide as well—that is, having formed the linear array, the helical structure follows from it. Figure 8 shows the linear construc-

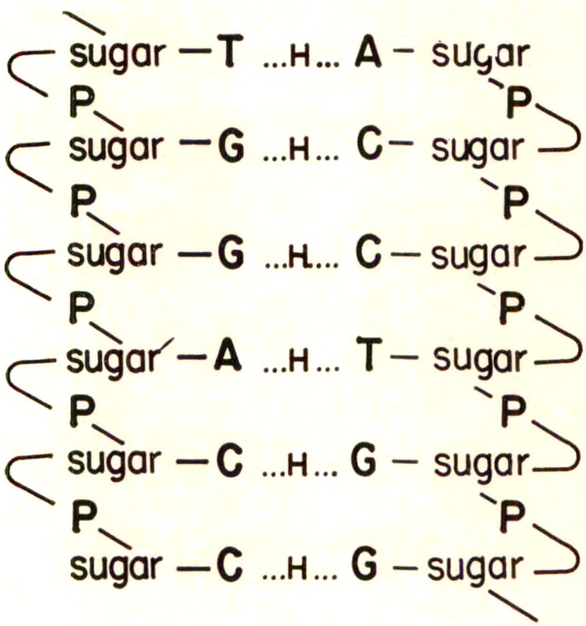


Fig. 9. Base pairing for DNA replication and RNA template formation.

tion of the polynucleotide. It is a 2-desoxyribose phosphate-3,5-polymer, and to each desoxyribose sugar molecule is attached one of the heterocyclic bases (thymine, cytosine, adenine, and guanine) by 1-glycoside amino linkage. The base pairs (A-T and C-G) each form a flat plane aromatic system, and the two polymer chains are held together by the hydrogen bond joining them. If the chains are twisted, a helix is formed as shown in Figure 9. The same sort of base pairing seems to occur for the ribonucleic acids as well as for the desoxyribonucleic acid.

It now seems clear that one of the principal functions of this latter simple type of linear polymer is the storage and transfer of information which is coded into the base sequence in the linear array [9, 19, 26].

While adenine, cytosine, guanine, and uracil are the principal bases in RNA, there are some half-dozen methylated bases as well which are present in trace amounts and which undoubtedly represent informational

marks along the RNA chain. There are probably a variety of rare special bases in the DNA as well, but this information is only now beginning to appear. One can see that the occasional presence of trace bases would give rise to much additional information in such a linear array.

Here, the double helical structure is something which is the permanent and stable form determined solely by the base sequence. One can

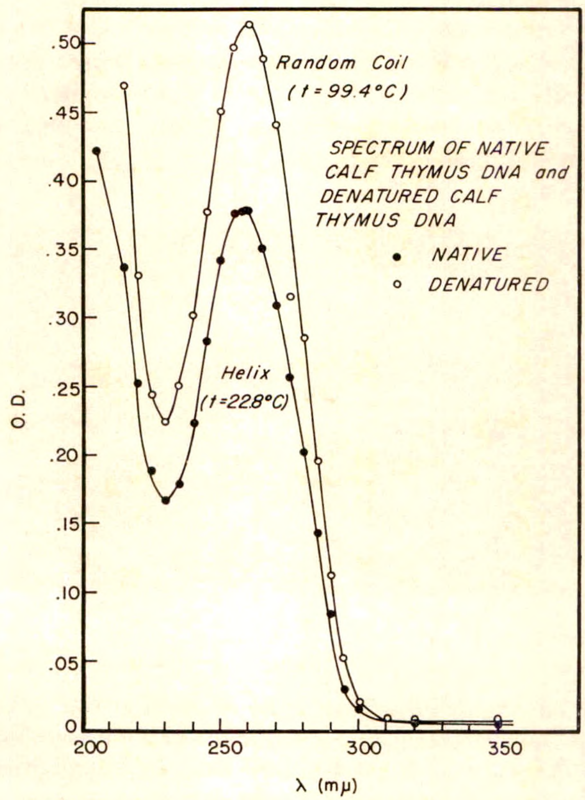


Fig. 10. Hyperchromism on nucleic acid [44].

demonstrate this in a fashion similar to that used for the polypeptide—disorganization with random coil formation, and recoiling (helix formation) as a spontaneous process depending on the thermodynamics of the situation. Figure 10 shows data for such a demonstration in nucleic acid. Here, at the absorption maximum at 2600 Å, the random coil has larger absorption than the helix [44]. One can go back and forth between the two types, in this case by simply changing the temperature. This is one more bit of experimental evidence to show that the structural information required for energy transfer in an ordered system [48], on the one hand,

and for information storage and transfer, on the other, are both thus contained in the linear structure of the corresponding polymers.

The Next Level of Organization: Finally, I would like to say something about the next higher order of structure reaching into the range of the visible—structures that can actually be seen either by electron or optical microscopy. Such structure also may be the ultimate resultant of the primary structure of the polymer. Figure 11 shows some collagen filaments. In the upper part of the figure they are separated into individual helices. If the proper types and amount of salt are added to a solution of these helices, they will aggregate, and collagen fibrils appear which look exactly like the natural collagen fibrils. The lower part of the figure shows some of the reconstituted fibrils. We are now getting into the visible region of structure.

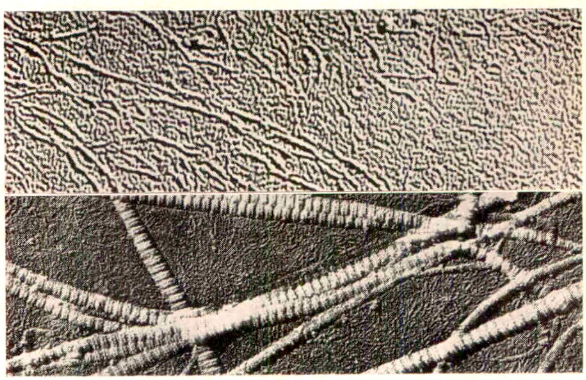


Fig. 11.

Thus, we have outlined a possible sequence of events to traverse the entire route from methane, ammonia, and water into visible biological structure. The point is that the information required to build visible biological structure appears to be contained in the electronic structure of the constituent atoms and the resulting molecular structure itself [5]. The possibility remains that some of the visible organizations of macromolecules (such as the lamellae of the chloroplasts) may themselves be the templates (analogous to crystallization nuclei) for their own reproduction. There is some suggestion of the existence of such nonchromosomal information transfer not only in the fact that, once lost from certain cells [39], they do not return, but in more subtle changes as well [42].

We will not discuss here the organization of these macromolecules (proteins, nucleic acids, carbohydrates) into cellular units, since experimental information is lacking. We know that such units exist and may even have a certain limited number of forms common to *all* living cells—

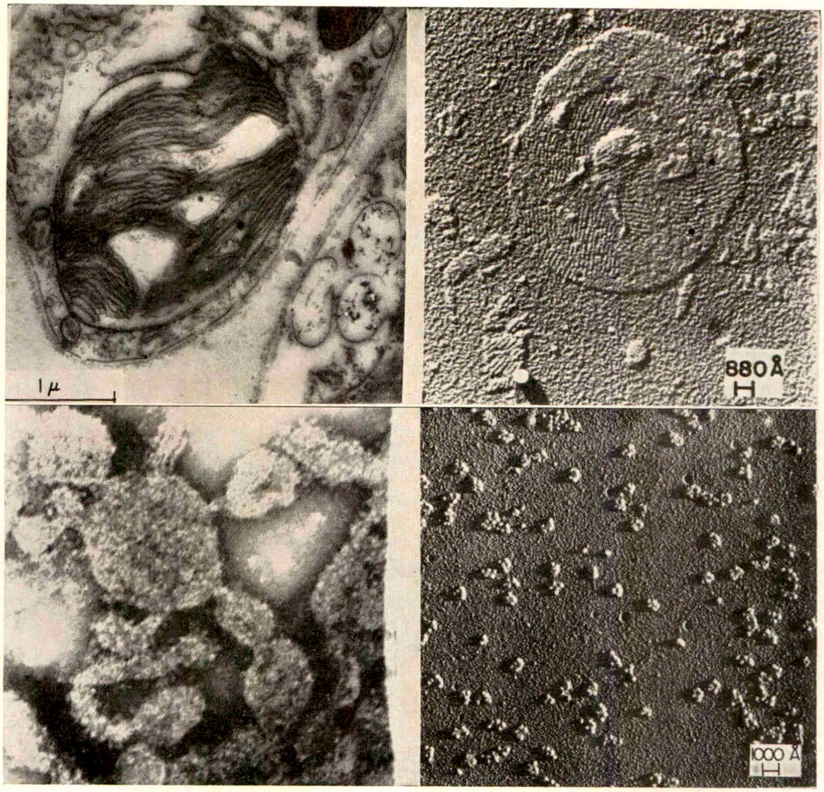


Fig. 12. Electron micrograph showing the "fundamental particles" of biology: ribosomes, electron transport particles of the mitochondria, quantasomes of the chloroplasts and unit lipoprotein membrane. (*a*) *Chlamydomonas* cells showing chloroplasts, mitochondria, ribosomes, and membranes [41]. (*b*) Spinach chloroplasts showing quantasomes [34]. (*c*) Negative-strained mitochondria [33]. (*d*) Polysomes making hemoglobin [46].

the "fundamental particles" of biology [31]. Figure 12 shows four of these units: the ribosomes, the electron transport particles of the mitochondria (more recently called oxysomes), the quantasomes of chloroplasts and the unit lipoprotein membrane so essential to the enclosure of the cell organelles as well as the cell itself. There is little information about the physics and chemistry of the organization of the macromolecules into closed, membrane-bounded packages which we call cells [50]. The gradual evolution of biologically active membrane structures from simple monomolecular films can as yet only be imagined and remains to be experimentally demonstrated.

Information Transfer

We have now arrived at the stage of enclosing the energy transfer and information communication apparatus within a cell wall. The next prob-

lem is to pass this structural and operational information from one cell to another. Here, we introduce two subdivisions of the information transfer process: (1) the *transcription* of information from one molecule to another, in which the language is still the same, i.e., simply passing knowledge from one place to another without using it; and (2) the *translation* of the instructions which may be contained in the transcription into

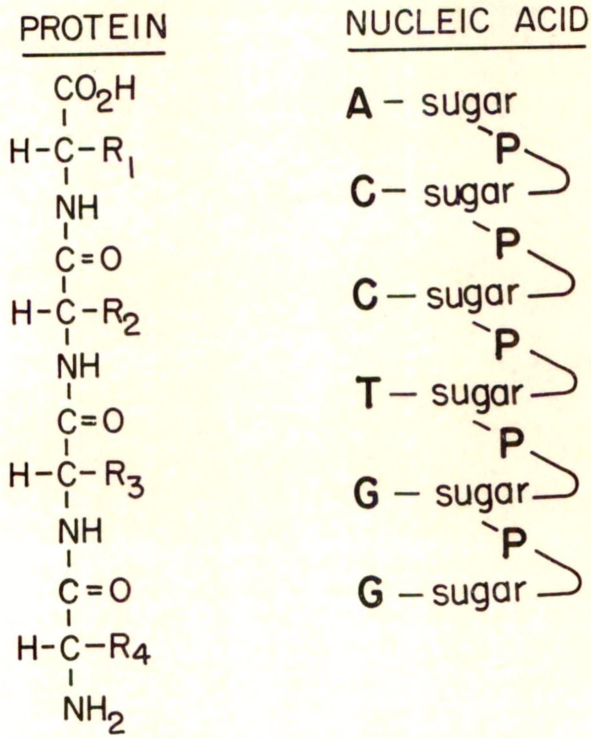


Fig. 13. Structure of protein and nucleic acid.

the construction of a new cell, i.e., following the instructions to create the machine which can manufacture a new set of instructions. The information may be transmitted from one cell to another, coded in a linear sequence of bases in a nucleic acid (i.e., transcription), and then that linear sequence of bases is translated into a linear sequence of amino acids which gives rise to the structure of the cell itself. How is this translation accomplished?

Figure 13 shows the two kinds of linear arrays: the bases in the nucleic acid which contain the coded genetic information which is handed on from one cell to another, and the proteins (used by the cell in structural and enzymatic functions) which require only the specification of a linear

array of amino acids. The coded transcription is made by zipping up another set of the nucleic acid bases complementary to the first one, following which the two strips are separated with one going to the daughter cell, thus accomplishing information transfer. The translation of one kind of linear array into the other is a much more complex operation. All sorts of information-handling machinery exist in the cell for this purpose, and the control apparatus which determines when to read, translate, and carry out a particular bit of the available instructions is only now becoming slowly known to us. In the last few years, it has become possible to begin

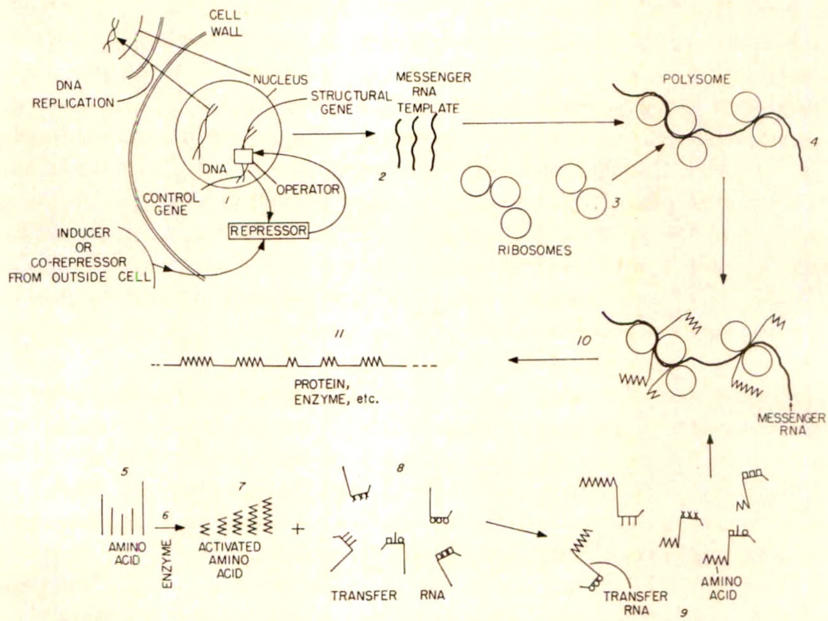


Fig. 14. A sequence of events of the molecular communication problem.

the first step in the compilation of the "dictionary" for the translation. How the actual translation is accomplished is more complex.

Figure 14 summarizes some current thoughts of how the translating mechanism may be regulated. In the parent cell, DNA replication (transcription) takes place by matching the bases in one helix to produce another polynucleotide which is then transferred to the daughter cell. The upper left-hand corner depicts the new daughter cell containing the new DNA which is now ready to be translated into the formation of a whole organism. How is the linear array of bases translated into protein molecules which are both structural and enzymatic? From the DNA, a linear array of complementary bases can be made which are hooked together by ribose sugar molecules into an RNA molecule, thus forming a

complementary template to the DNA or some particular part of it. This template which is made in the cell nucleus, and which presumably comes out of the nucleus in some unknown fashion, is called "messenger" (template) RNA. It is the material which reads the coded message off the nuclear memory and carries it to the translating and construction apparatus in the cell cytoplasm, enabling it to make the proper material. The messenger RNA is a linear sequence of bases corresponding either to the whole or part (we believe it is part in the higher cells but it may be the whole nucleic acid in the simple viruses) of the genetic nucleic acid. The "factory" or "assembly line" is a combination of nucleoproteins which is in a small particle, about 200 Å in size, the ribosome.

It is now quite clear that it is not possible to get the rate of construction that is necessary with only one ribosome at a time working on a single messenger RNA molecule. The situation now appears to be that the messenger RNA can have several ribosomes rolling along it simultaneously [46, 47]. The ribosomes contain various amounts of polypeptides, and if the RNA messenger has information for the construction of several proteins, presumably there are certain punctuation marks along it which induce the detachment of the ribosome with its completed protein molecule for release. The protein molecule, having come free, folds up into its secondary and tertiary structure and takes up its function. The ribosome then goes back to pick up more messenger.

Correspondingly, at each one of these punctuation marks an entire synthetic apparatus begins. Recently I have seen electron micrographs of polysomes, which are collections of seven or eight groups, which start at different points along the messenger [8, 40], each one of these points presumably being punctuated in some way, as yet unknown. The messenger evidently is making many things simultaneously.

How do the ribosome and messenger collaborate to make a polypeptide of a particular variety? Here, it is necessary to have a *translation* mechanism. Up to this point the DNA has only been *transcribed* into RNA; the *translation* must now be accomplished. The amino acids in Figure 14 (5) may come in from the medium outside the cell, or be synthesized within it. They are transformed by enzymes (Fig. 14 (6)) into activated amino acids (7); the special enzymes which do this seem to form an enzyme ester, generally on the carboxyl group of the amino acid which is then transferred to specific small molecules of what is called "transfer" (or soluble, i.e., s-RNA) RNA (8). This molecule has a very specialized character; it is small, only about eighty bases long. Each of the s-RNA's has somewhere on it a three-base sequence which corresponds to a specific amino acid. While the literature suggests that the transfer RNA which is made up of some eighty bases is a hairpin-like structure whose ends form a complementary double helix, this has recently been called into question [8]. However, for the moment let us accept this hypothesis. The

s-RNA contains three bases, presumably at the bend in the hairpin, which are not paired. These unpaired bases have been called the "codon" for a particular amino acid. The special enzyme (6) to activate the amino acid which is transferred to the specific s-RNA containing the specific codon also has amino acid specificity. Several of the various transfer RNA's have been isolated as pure substances—alanine transfer RNA, serine transfer RNA, etc.—and work is rapidly progressing now toward the determination of the complete base sequence in the transfer RNA's. There may be two or three codons for one amino acid, but it is clear that there are differences in the "handle" structure of the different transfer RNA's from different organisms [8].

This transfer RNA (s-RNA) is really the translating mechanism within the cell. The relation between the three bases and the amino acid is contained in the transfer RNA. The three bases match up with the corresponding three bases in the messenger RNA and thus put the amino acids in the right sequence as directed by the messenger. The amino acids, thus suitably activated and placed, then "zip up" and the proper protein emerges, by an as yet unknown mechanism.

There must exist a control apparatus within the cell that determines which parts of the DNA should be read at a given time. Every cell of a particular organism presumably contains the same kind of DNA (genetic material) but every cell does not manufacture the same things—the cells that make the brain make different things from the cells which go to make up other organs and tissues such as fingers, liver, etc. This is the basic problem of control of growth and differentiation. How do the different cells know that they have different functions? What tells the individual cells what parts of the DNA to read? Here must operate the control mechanism which determines how a cell behaves even though its genetic constitution is predetermined by the base sequence of its inherited DNA. How the genetic constitution of the cell is to be expressed, when and in what order, is determined not merely by the inherited DNA but by the *environment*. Here we come to a point at which social evolution, the control of evolution by man, can really take hold, certainly on a cellular level and probably on an organismic one as well.

From Chemical to Social Evolution

We are now just beginning to learn the mechanisms which control the way in which a cell can develop. It is the variety in this development which can give rise to a brain cell, an eye cell, and perhaps a cancer cell, etc., all from the same initial cell. Of more direct and immediate concern is what happens if the cells go wild, as they do if the control mechanism is faulty, and they become malignant. We are here in the region of theory based upon a combination of bacterial and virus genetics, on the one hand, and some enzyme chemistry on the other. The control of the read-

ing of the DNA is exercised through, or can be influenced by, something from outside the cell. For example, a small molecule outside the cell can determine whether a certain particular part of the DNA molecule inside the cell can be transcribed into messenger RNA or not. This promises to give us a handle on the control of development.

If we can already do some of these things with one type of material and organism (bacterial induction, lysogenic viruses, and transduction) it is not an improbable extrapolation to believe that, as chemists, we can make a large variety of materials, some of which could, for example, produce a new or abnormal type of organism. At birth, the human has a certain number of brain cells, about 10^{10}, which is normally all it will ever have. The brain cells make a great number of connections—excitatory, inhibitory, etc.—which are the basis for behavior of this computer which is the human brain. If it be possible to control the growth of various developing cells in the embryonic brain (and there should be chemicals which can accelerate or decelerate the growth of certain specific kinds of cells), it is quite clear that we might change their number or at least their distribution. If the computer is limited by the number of connections it can make, and if one could go from 10^{10} to 10^{11} brain cells (this would, of course, require an adjustment of cranial or cell size), there is a chance that the capacity of the brain could be increased. This is theoretically now within our range.

We are approaching not only the means of selectively transforming the gene but, what is even closer, the means of deciding which genes to read and which ones not to read, and how long to read them. What effect might this have on social evolution?

Social evolution, on a physiological level, until now has been determined primarily by the same processes of random mutation and selection that gave rise to the human race in the first place. We now have coming into our hands the tools for the control of genetic information itself, and closer still may be the ability to control the genetic expression of information which is in any existing cell. This would not entail any change in the information (mutation or recombination) but merely to control how it is used.

On the bacterial level both things have already been done; transduction in microbes has been achieved. One can introduce genes into the chromosome of a bacterium (almost at will) which can be incorporated eventually into the bacterial chromosomes. This is what happens with lysogenic viruses; they get into the cell and remain there, and eventually some of them do get attached to the chromosome and become part of the bacterial chromosome. This is changing the bacterial chromosome by introducing new information. More easily done is the control of the expression of the existing bacterial gene by simple molecules [23, 38] from

the environment itself. These can penetrate into or out of the cell almost at will and can, in turn, exercise a controlling function on the ability of that cell to express its genes.

Through this mechanism it may be possible for us to control virus disease, cancer, and perhaps even change the adaptability of men, thus leading to directed social evolution. The moment we start thinking about things of this nature, we cannot escape the enormous problems involved. Who is going to decide to change men, and how many of them, and in what way [6, 7]? This is a problem which we, I believe, will ultimately face and we should begin thinking about it now.

One of the most far-reaching developments in social evolution will come about from this new knowledge of the manipulation of the basic polymeric materials of which all living substance is composed. We are learning the chemical composition of the genes, and their constituents, the chromosomes, and their structural arrangement. We are learning how to alter genetic material deliberately to produce types with predetermined characteristics. This is being done with microorganisms in the laboratory right now. But, in the future, as our knowledge grows, we should have a corresponding power with plants and animals and man himself.

Two aspects of this situation should be considered from the human point of view. Many of the studies of genetic material are being carried out in the interest of controlling virus diseases and cancer. There is little doubt that eventually success will be achieved. The same genetic knowledge will contain the information we need for controlling both the "quantity" and the "quality" of the population. We may have the power to intensify certain human traits, delete others, and perhaps even develop new ones [7]. An important corollary of this is the approaching ability to control men's minds by chemical means, bringing with it the major problem of how and by whom this power should be exercised [6, 7].

The distance from Atom to Adam covers billions of years. By following the laws of the behavior of matter, the process has been orderly, even in its infinite complexity. But, during these years, the laws of nature have functioned in a laboratory in which each atom had its destiny, but within which no encompassing comprehension of the whole could sway the course of experiment.

Today, the world is quite as awesome to contemplate as it must have been in its beginnings, for today man is here and he has a little knowledge! With each thread of new truth, the responsibility to weigh the consequence of its application becomes more critical. The rate of evolution can change tremendously with man's new knowledge, and the responsibility to control the rate and the direction of change must depend on wisdom. As it has to this day, time will record our success—or our failure.

Summary

An answer to the question of how the molecules which constitute today's living organisms may have arisen on a prebiotic earth is being sought within the context of modern experimental science. We begin with the primitive atmosphere as it is presently conceived by a consensus of astronomers and geochemists, namely, a reducing one, and introduce various forms of energy into this system to determine the nature of the molecular changes which might occur and which do occur. Experimental demonstration shows that the atoms which constitute the primitive atmosphere are of such chemical character that they give rise to molecules of biological interest almost immediately under these conditions. Autocatalytic mechanisms, beginning with the crude catalytic properties of the mineral surface of the earth, then select among these molecules certain classes as favored.

The basic problem of the generation of macromolecules of two general types is discussed. The first, resulting from carbon–carbon linkage, comes via vinyl polymerization. The second, resulting from dehydration condensation, has been more difficult to demonstrate experimentally as possible in an aqueous medium. However, certain dehydrating agents are now being discovered which show signs of functioning specifically in the aqueous milieu to give rise to the protein, nucleic acid, and carbohydrate types of polymers.

Then the question of a higher degree of order, leading ultimately to visible structure resulting from the construction of macromolecules is discussed. It is shown that a sequence of thermodynamically controlled processes may be expected to give rise to secondary, tertiary, and even quaternary structure in such systems, the last eventually reaching the range visible under suitable microscopic conditions. The question of membrane formation and boundary enclosures is still moot.

However, the evolution of macromolecules, according to present laws of molecular evolution, is now visible to us. These laws can be seen to lead to the kind of organization we now recognize as living, and new chemistry is daily derived via the attempt to understand and reproduce such systems.

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TASTE, ITS SENSORY AND MOTIVATING PROPERTIES*

By CARL PFAFFMANN

GUSTATION, the sense of taste, is of interest on several accounts. First, it is a receptor sensitive to different ions and molecules, so that it poses the question, *a*, of the molecular basis of chemical selectivity in the taste cell and, *b*, how that selectivity gives rise to an afferent neural code in the taste nerves to the brain resulting in the sensations of salty, sour, bitter, sweet or some other quality. Secondly, taste may control ingestive behavior, that is, the acceptance or rejection of foodstuffs and other substances. Some, like sugar, are accepted avidly by most creatures; others, like quinine, are strongly rejected; while still others are relatively neutral. How does the sense of taste and its associated neural processes come to mediate the positive response of acceptance, on the one hand, or the negative response of aversion, on the other? Thirdly, taste may initiate specific hungers or intense cravings for particular substances that are essential for well-being, thus contributing in some measure to the "wisdom of the body." In what way do the taste buds or the taste centers of the brain react so that needed substances become more detectable or more palatable? Finally, taste stimuli in their own right may serve as rewards or, more exactly, as reinforcing agents for new learning. Many different responses may be acquired simply because they are reinforced with a drop of "sweetened" water. The specific behavior so reinforced, however, may be quite unrelated to the act of ingestion itself. The sense of taste, therefore, provides the experimental scientist with a stimulus-response system in which he can hope to uncover some of the psychophysiological mechanisms underlying the sensory control of behavior.

"Taste," as I shall use the term, will have its exact denotation as compared with common usage, which equates "taste" with flavor, the perception complex which often includes smell, texture, temperature, and sometimes pain as well as sensations of true taste. Highly spiced Mexican food would be insipid without the contributions from buccal pain. But the unique qualities of salty, sour, bitter, and sweet aroused in man by sodium chloride, mild hydrochloric acid, quinine, and sucrose, respectively, derive only from stimulating taste buds.

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The Neurophysiology of Taste

Figure 1 shows a taste bud in histological cross section. The taste bud is a cluster of 20 to 25 sensory cells in the form of a goblet or "brandy snifter." The taste cell nuclei are darkly stained, whereas the distal portions projecting up to the gustatory pore are more lightly stained. Stimulating solutions reach the taste cells through this pore.

Taste buds occur primarily on the tongue and posterior buccal cavity; in children they may be more widely scattered on the insides of the cheeks

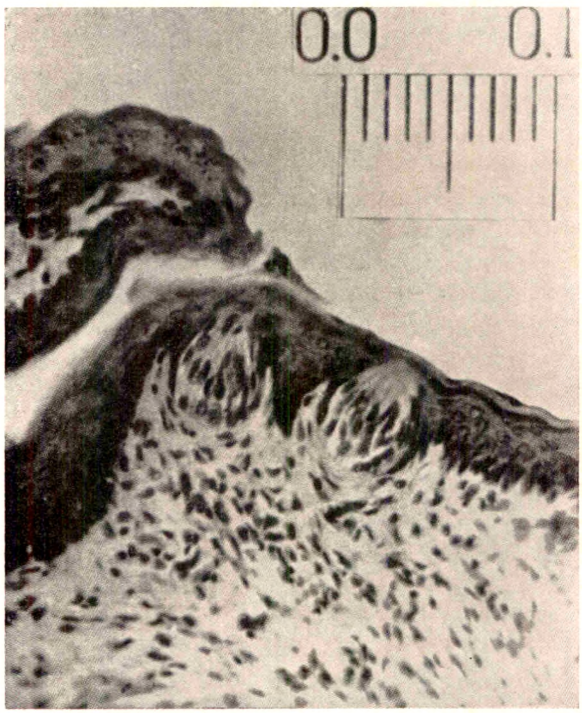

Fig. 1. Histological section of two taste buds from the naso-incisor papillae of the rat. (Reproduced by permission of *J. Comp. Physiol. Psychol.*, Pfaffmann, 1952.)

and pharynx. Some organisms, like the rat, have taste buds on the hard palate, in the neighborhood of the naso-incisor papillae, as shown in Figure 1. Certain fish have taste buds scattered over the external body surface so that they literally swim in a "sapid sea." Some insects possess chemoreceptors on their feet and taste nectar by walking in it.

On the tongue of mammals, taste buds are located in the mushroom-shaped fungiform papillae of the anterior dorsal surface, in the grooved foliate papillae toward the back of the tongue on either side, and in the circular trenches of the vallate papillae on the posterior surface. The lo-

cation of fungiform papillae on the rat's tongue can be visualized by injecting methylene blue vital dye into the lingual circulation to stain darkly the vascular plexus beneath each papilla (see Fig. 2). In the living animal, these spots appear bright pink. Man can see his own fungiform papillae in a mirror as bright pink spots on the tongue. In the rat, such papillae usually contain just one taste bud each. In Figure 2, the foliate papillae on the sides of the tongue are just barely visible but the vallate papilla, which in the rat is a single organ, can be seen near the base of the tongue. In man, seven to nine circumvallate papillae are arranged in a chevron-like row on the back of the tongue. In both the foliate and val-

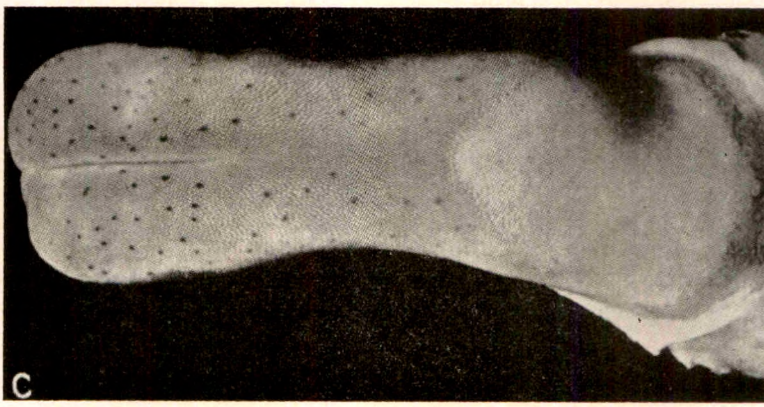

FIG. 2. Distribution of papillae on rat tongue. (Reproduced by permission from *Anat. Rec.*, Fish *et al.*, 1944.)

late papillae, many taste buds are found buried in deep trenches, so that mechanical stirring and agitation are needed to insure stimulation. The total number of taste buds in man is of the order of 10,000.

Most special senses are supplied with a single cranial nerve from receptor to the brain. For example, the optic nerve mediates vision, the acoustic nerve, hearing. Taste, however, has a multiple nerve supply, the chorda tympani nerve for the front of the tongue, the glossopharyngeal nerve for the back, and the vagus nerve for the deeper recesses of the throat and pharynx.

The nerve fibers subserving touch, temperature, and pain coursing beneath the epithelial layer of the mucous membrane terminate at various levels in the mucosa. Taste nerve fibers enter the base of the taste bud and branch among the sensory cells; some terminate within deep infoldings of a single sense cell while others end in the spaces between two or more sense cells. Still others appear to extend upward almost to the gustatory pore itself. In each case, the receptor cell and its nerve fiber is discontinuous; that is, the contact is a surface to surface synaptic-type junction. The molecular events by which taste solutions stimulate the

taste cells is not yet understood although adsorption of ions or molecules at specific receptive sites on the microvilli appears to be an important initial step. Upon stimulation, the sense cell is depolarized in proportion to the logarithm of the concentration of the taste solution (Beidler, 1962). This depolarization in turn excites the sensory nerve fibers, giving rise to a train of nerve impulses that travel in an unbroken neural pathway to the first sensory relay in the brain stem and then on to the higher centers beyond.

Because the taste nerve fibers are of relatively smaller diameter and fewer in number than those concerned with touch, temperature, and general sensitivity, it is difficult to record taste-initiated activity in the mixed nerves from the tongue. But nearly all the taste fibers branch off from the lingual nerve deep in the neck just beneath the angle of the jaw. After leaving the lingual nerve, this small branch, the chorda tympani nerve, dives through a bony canal into the middle ear cavity, where it passes across the tympanic membrane, hence its name. It then enters the brain through a second bony canal. The chorda tympani also carries efferent secretory impulses in the reverse direction to the salivary glands.

The sense of taste on the front half of the tongue on the same side is lost when the chorda tympani nerve is cut or damaged. This not only eliminates the pathway from the receptors to the brain but is followed by degeneration of nerve fibers distal to the cut and a disappearance of taste buds. Histological examination of the tongue papillae shows no signs of the taste bud. Later, if the nerve fibers grow back to the tongue, taste buds are reconstituted. Apparently, the taste nerve exerts a trophic influence so that epithelial cells are modified and develop into receptor cells. Actually, such decline and regrowth after nerve section only exaggerate a process that is continually present during the natural life of the taste bud. Classical histologists classified taste cells into two types: (*a*) long, slender sensory cells, measuring in man 60 to 80 microns in length and 40 microns in diameter at the thickest part, and (*b*) thick supporting cells. Some (Kolmer, 1927), however, suggested that these might be only different functional stages of but a single type of cell. Recent studies (Beidler, 1963; de Lorenzo, 1963) indicate that the taste buds are continuously being replenished with new sensory cells by continuous mitotic division at the edges of the bud. The injection of a drug, like colchicine, blocks mitotic division and causes a rapid atrophy of taste buds with consequent sign of loss of function. Radioactive tracer compounds, like tritiated thymidine, can be used to label those regions of the taste bud in which cell division and the formation of new nuclear materials may be going on. These studies indicate that the taste sensory cells are being continuously formed at the boundaries of the taste bud and that, with time, the labeled cells migrate toward the center of the bud and then die out. The turnover cycle of sensory cells occupies a seven-day

period. But, in spite of such a continuous turnover, there is a stable manifold of taste sensations. Throughout this process, it should be emphasized that the nerve fibers themselves do not die out. They are the constant factor; only the receptor cells wax and wane.

Electrical stimulation of the chorda tympani nerve itself may give rise to taste sensations which are referred to the front of the tongue of the same side. Solutions of salt, sugar, or quinine on the tongue produce an increase in taste nerve impulse traffic. This has been registered in man when the chorda tympani nerve was exposed during middle ear surgery

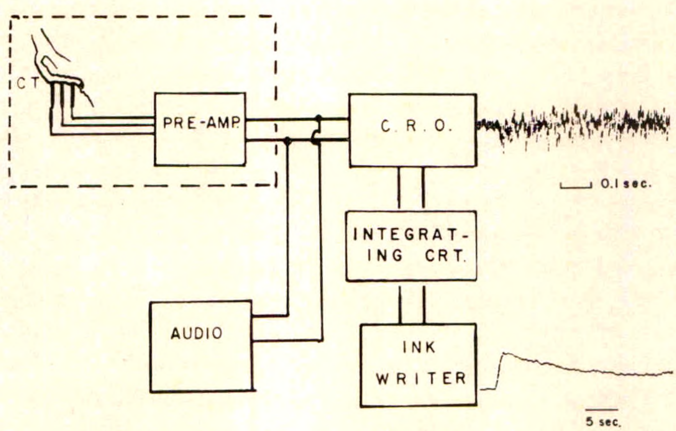

FIG. 3. Oscillographic and summator write-out method. (Reproduced by permission from *Amer. J. Clin. Nutr.*, Pfaffmann, 1957).

(Diamant, Funakoshi, Ström, and Zotterman, 1963). Actually, this procedure is an adaptation of that first used in animal preparations, where it has been used extensively for more analytical studies (e.g., Beidler, 1953; Pfaffmann, 1941; Zotterman, 1935).

The impulse discharges in man and animal are very similar, although there are marked species differences both with regard to the relative effectiveness of different stimuli and the magnitude of the electrical activity so produced. The electrical signal from the total nerve represents the discharges in a large number of nerve fibers, all of which fire at the same time but in asynchronous and random manner relative to each other. Interpretation of the oscillogram of such a record is aided with a summating or averaging device which provides a quantitative record of the rise and fall in activity, shown in Figure 3. The stronger the concentration of the taste solution, the greater the recorder's deflection. Accordingly, it is possible to draw a graph showing neural response magnitude as a function of stimulus concentration. Figure 4 illustrates one such graph for the white rat, an animal we have used in

many studies. The response to the quinine stimulus becomes apparent at the lowest concentration and rises gradually with an increase to a value of -1 on the logarithmic scale of concentration. The responses to HCl and NaCl both begin at a higher value although HCl is more effective than in equimolar concentrations of NaCl. The threshold for the sucrose is the highest of the four. In every case, the curve of magnitude of neural discharge tends to be a sigmoid function of concentration. The greater magnitude of response to the two electrolytes in this figure presumably indicates that the taste buds and nerve fibers activated by quinine and sugar are relatively fewer than those subserving acid and salt. In other species, such as the hamster, the magnitude of response to the

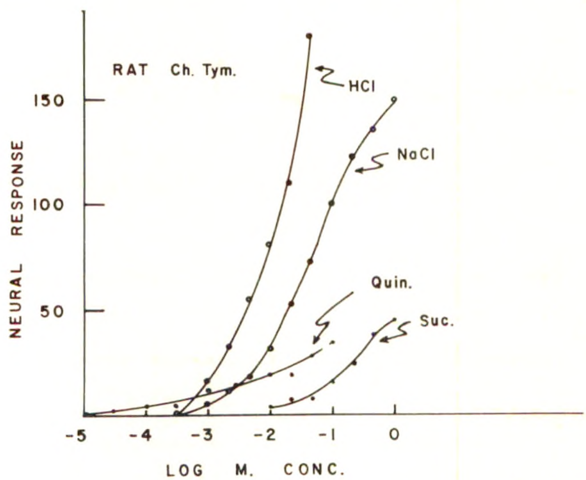

FIG. 4. A graph of taste sensitivity in the rat chorda tympani to four basic taste stimuli. (Reproduced with permission from Koch, *Psychology: A study of a science*, McGraw-Hill, Pfaffmann, 1962.)

sugar stimulus may be as large as that to the salt. In the cat, the response to the quinine is usually quite large. Thus, there appears to be a species difference in the distribution of different sensitivities over the tongue. Since the potential produced by any one nerve fiber is determined by its diameter, some of the differences in magnitude for quinine and sugar reflect differences of their respective nerve fiber diameters.

The activity of single nerve fibers can be studied by dissecting these nerves under a binocular microscope with the aid of sharpened needles. Since the nerve impulse in any one fiber obeys the "all-or-none" law, the nerve impulse from a single nerve is always of uniform potential and, at the recording electrode, is reflected by spikes of uniform height on the oscillogram. In addition, there is no "doubling up" of impulses one upon the other, so that the single unit record has a characteristic uniform spike height, duration, and waveform. Such a record is illustrated in Figure 5.

As the strength of the stimulus increases, there is an increase in discharge frequency. Furthermore, the receptors on the tongue constitute a population of receptors of varying theshold. Thus, more receptor units will be excited by stronger stimuli. Intensity, then, is signaled both by an increase in the numbers of fibers active as well as by the frequency of discharge per fiber once it has been activated.

The Afferent Code for Taste Quality

Of particular interest is the chemical selectivity of single receptor-neural units. According to the classical view, there were said to be four basic taste receptors, each giving rise to the sensations of salty, sour, bitter, or sweet when activated by its appropriate stimulus. The salty

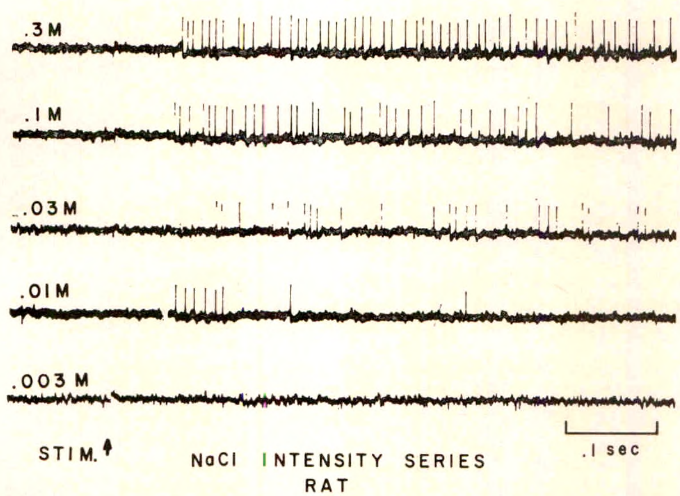

FIG. 5. The response of a single test nerve fiber in the rat chorda tympani to a series of different NaCl concentrations.

taste was said to result from activating a specific "salty" receptor-neural unit by salt, sour from a specific "sour" receptor by acid, etc. Our earliest experiments were begun, in fact, as a search for these specific receptors using the direct electrophysiological method in animal preparations. But this study of single gustatory afferent fibers (Pfaffmann, 1941) failed to support the classical theory, for although single units in the cat's chorda tympani were selectively sensitive to different chemicals, the observed types of sensitivity cut across the basic categories. In the cat, one set of receptor-units responded to acid, a second group to *both acid and salt*, and a third to *both acid and quinine*. More recent studies have revealed other clusters of mixed and multiple sensitivity in other species (Cohen, Hagiwara, and Zotterman, 1955; Pfaffmann, 1955). In addition, many species possess receptor neural

units (Zotterman, 1956) which are stimulated by water and inhibited by sodium chloride and other salts.

Combinations of sensitivity such as these might result from (*a*) multiple termination of individual gustatory afferents upon two or more highly specific sense cells, (*b*) a truly multiple sensitivity of the individual primary sense cells themselves, or (*c*) a combination of these two. Kimura and Beidler (1956, 1961), recording the depolarization receptor potentials of individual taste cells with intracellular micropipette electrodes, showed

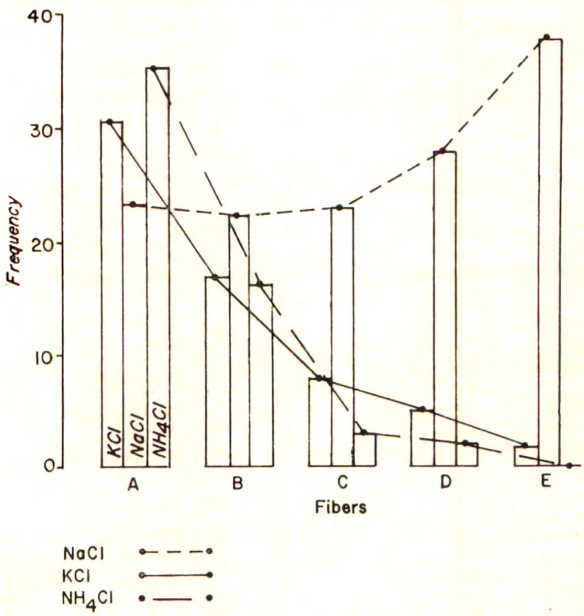

FIG. 6. The nerve impulse profiles across five different single nerve taste fibers to three different salts based on data by Erickson, 1963. (Reproduced with permission of Gerard, *Processing of information in the nervous system*, Excerpta Medica Foundation, Pfaffmann, 1964.)

that the individual cells possessed combinations of sensitivity like those seen in the afferent fibers. Hence, the patterns of sensitivity in the chorda tympani sensory nerve fibers do not result merely from multiple terminations upon several afferent cells. Individual receptor cells apparently contain many individual sites with a variety of different molecular specificities. These may occur in different combinations upon the single receptor cell. Chemical selectivity must be sought at the molecular level of the individual receptor cells.

The first step in stimulation appears to be adsorption. Differences in the configuration of the receptor surface probably account for the selective adsorption of one or another type of chemical at any one site. Just as an ion exchange column or polyelectrolyte surface may be tailored for

the adsorption of different ions, so it is conceivable that the different receptor surfaces are specialized for adsorption and ultimate stimulation by ions or molecules of different size, charge, or molecular configuration (Beidler, 1962).

The different combinations of molecular specificity on any single receptor cell scramble the gustatory afferent input to some extent and this must be decoded by the central nervous system. One possible coding mechanism is exemplified by Figure 6, based on data obtained by Dr. Erickson (1963) at Duke University. The figure shows the frequency of discharge recorded in a sample of five single afferent rat nerve fibers

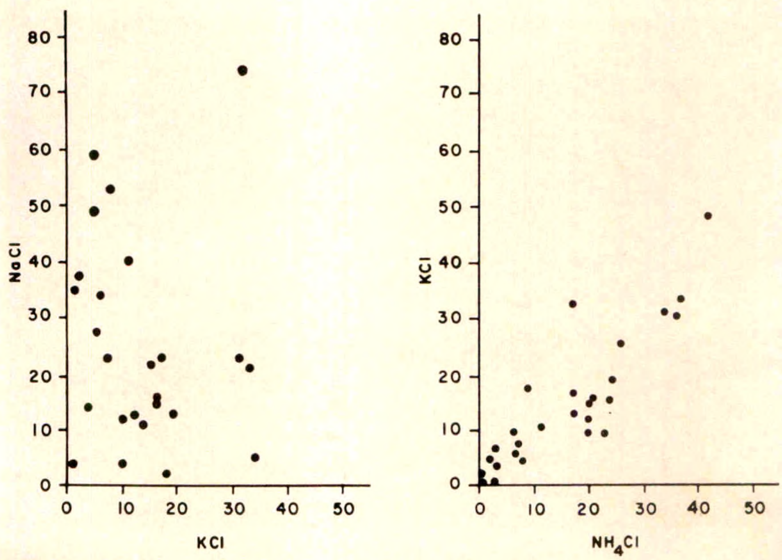

FIG. 7. Scatterplots showing the correlation in discharge frequencies in the same single chorda tympani fiber to three salts based on data from Ereckson, 1963. (Reproduced with permission from Gerard, *Processing of information in the nervous system,* Excerpta Medica Foundation, Pfaffmann, 1964.)

obtained by microdissection. The response to three different electrolytes, NaCl, KCl, and NH_4Cl, is indicated by the heights of the vertical bars in units A through E. By joining the tops of these bars, we may construct a frequency profile of response. The profile for NH_4Cl resembles that for KCl, for both stimulate the individual receptor-neural units A through E to the same degree. The profile for NaCl, however, is different; high KCl frequencies may or may not be associated with a high NaCl discharge. To say it another way, the frequencies of discharge for NH_4Cl and KCl in different afferent fibers are positively correlated. The response frequencies for NaCl and KCl, on the contrary, are uncorrelated.

These five fibers are only a smaller sample of the population analyzed

by Erickson. The degree of similarity in discharge profiles can be described by the product-moment correlation coefficient between the frequencies of firing of the same units for different stimuli (see Fig. 7). In the rat, the frequency profiles for NH_4Cl and KCl yield a Pearson r of $+ 0.88$, whereas between KCl and NaCl $r = -0.09$, indicating that the sensitivity for NH_4Cl and KCl rises and falls together; that for NaCl bears little relation to either NH_4Cl or KCl. If we make an assumption that the discrimination between stimuli is related to such frequency profiles, then highly correlated stimuli should taste alike; uncorrelated stimuli should taste differently.

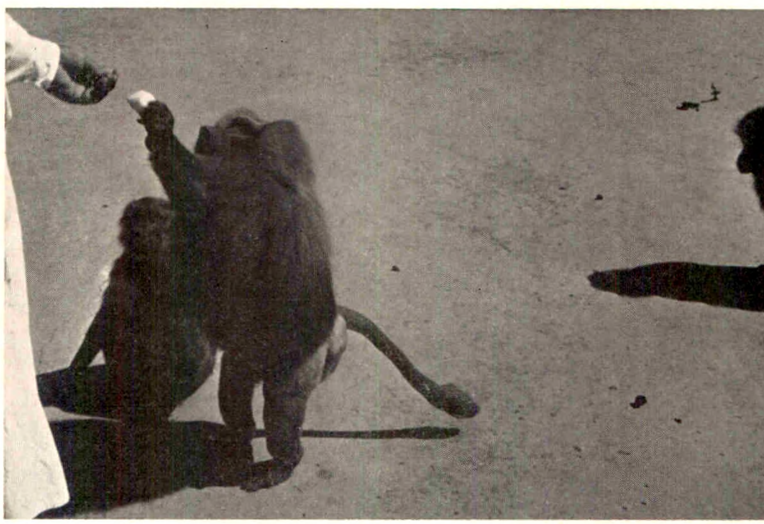

FIG. 8. The "sweet tooth" displayed by a baboon at the Sukhumi primate station.

This purely neural model can be subjected to tests by means of conditioned response tests. It is well known that once an animal has been conditioned to one stimulus, it will display a tendency to generalize, that is, to give the response to other stimuli that resemble the original conditioning stimulus. Using a shock avoidance procedure, Erickson (1963) showed that rats conditioned to avoid KCl in a drinking test also tended to avoid NH_4Cl but not NaCl. Conversely, the depression in drinking after being shocked on NaCl generalized somewhat to the other two salts but no more to one than to another. The generalization test indicates that, for the rat, the tastes of KCl and NH_4Cl are similar and that both of them differ from the taste of NaCl. Generalization in the behavioral test supports the predictions from the frequency profiles in the chorda tympani nerve fibers.

The discharge profiles of the rat's chorda tympani do not conform to

any obvious typology; rather, some sensory units are sensitive to a wide range of stimuli and others to a more narrow band. In addition, the "band width of sensitivity" is influenced by the stimulus concentration level so that what appears to be a "sugar unit" at one level may be a "salt-plus-sugar unit" at a higher concentration. Activity in any taste fiber, taken alone, cannot indicate unequivocally which of several different chemicals had been applied to the tongue. Concomitant activity in several parallel fibers, therefore, seems necessary to give the full meaning to the discharge from any one unit (Pfaffmann, 1959).

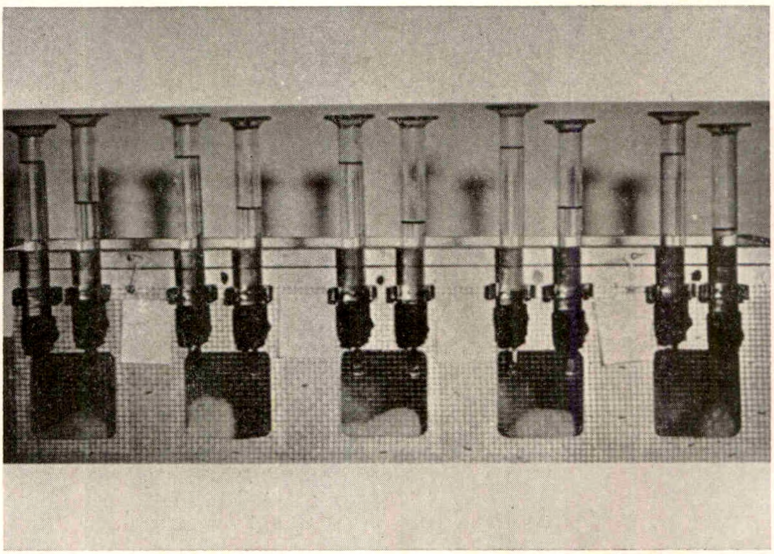

FIG. 9. A battery of five preference test cages. Sugar is contained in the right cylinder, water in the left of each cage (as seen by the observer).

It is important to remember that many different models of the sensory code for sensory quality might be developed solely on the basis of physiological data. In the studies just reviewed, there is a unique validation of the model by the outcome of behavior tests generalization or similarity. *Similar experimental strategies should be used more widely in attempts to relate neurophysiology to behavior.*

Motivating Properties of the Sweet Taste

We shall now turn to behavioral studies which warrent the conclusion that taste stimuli are reinforcing or motivating in their own right. Figure 8 is a photograph I took a couple of years ago on a visit to the Primate Biomedical Research Station at Sukhumi in the Soviet Union. Here, a large colony of baboons lived in an open hillside range enclosure.

In the picture, the dominant male of the colony is shown begging for a sweet "tidbit" from the hand of the curator, who always kept a generous supply of lump sugar in his laboratory coat pocket. This is but another example of the "sweet tooth" so widespread throughout the vertebrate and invertebrate species. In a few cases, this appears to be nearly absent. Cats, when not hungry, seem unable to discriminate sugar solutions from water, in agreement with their relatively small response to sugar in the chorda tympani nerve discharge. Chickens, likewise, seem unreactive to sugar solutions (Kare and Ficken, 1963). But these are more the exception than the rule.

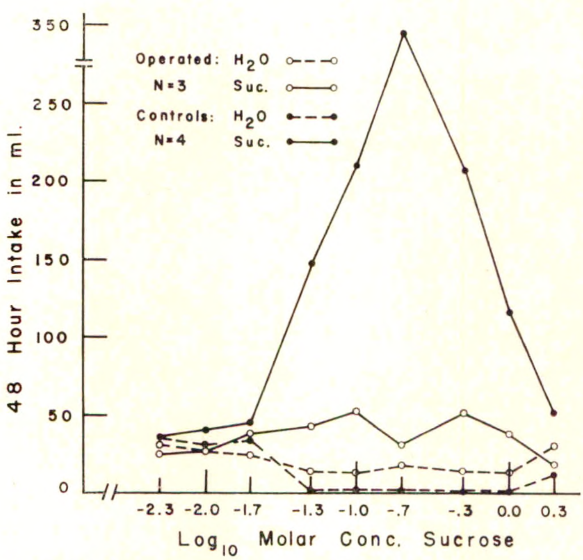

FIG. 10. Graph of the preference for sugar solutions of ascending concentration in normal and taste desensitized (operated) rats (Reproduced by permission from *J. Comp. Physiol. Psychol.*, Oakley & Pfaffmann, 1962).

The response to sugars can be studied systematically by the two-bottle preference technique originally developed by Curt Richter (1942) a number of years ago. Figure 9 shows five individual rat cages, each with a pair of graduated drinking cylinders, at the end of a 24-hour period of *ad lib* diet and free choice between water and sugar solution. The sugar solution, in the right-hand cylinder, is preferred over the water, on the left. That this response to sugar is dictated by taste can be established by desensitizing taste and then measuring preference again. Although this can be done by cutting the nerves in the tongue, another method can be employed (Ables and Benjamin, 1960; Oakley and Pfaffmann, 1962). Electrodes were inserted bilaterally by means of a stereotaxic device into

the sensory thalamic area for taste and the tissue coagulated by passing a mild electric current. The electrodes were withdrawn and the animal allowed to recover. Figure 10 shows a graph of the two-bottle preference response to a series of sugar solutions in a desensitized and a normal group. The solid circles and solid line show the normal animal's very large intake of sugar in the middle range of concentration compared with the much reduced sugar preference of the animal with a thalamic lesion. Although the nutritive value of the sugar solution was the same in both cases, the interference with taste sensitivity produced a very remarkable change in behavior. The experimental animals shown here did

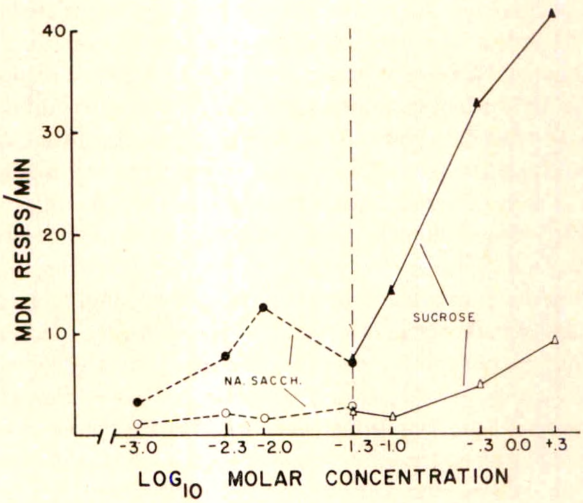

FIG. 11. The rate of bar pressing for saccharin and sugar solutions of different concentrations in normal and taste desensitized rats (Oakley, personal communication, 1963).

not completely lose the preference for the sugar; perhaps taste desensitization was not complete. The essential observation, however, is clear: taste desensitization *markedly* reduces the sugar preference. Similar results are observed in the quinine aversion, which is reduced after thalamic lesions (Benjamin and Akert, 1959; Oakley and Pfaffmann, 1962; Patton, Ruch, and Walker, 1944).

These studies show only that ingestive behavior is influenced by taste. But taste can motivate the acquisition of other responses. If a rat is placed in a Skinner box, a small enclosure under uniform environmental conditions, the animal readily learns to obtain a small reward by pressing a bar which projects into the cage. Either a pellet of food, if hungry, or a drop of water, if thirsty, is effective. *But the rat will similarly learn to press the bar for a drop of sugar solution or saccharin when not hungry or thirsty.*

Many another response might be used; the rat might be taught to go to the right or to the left in a T-maze for the taste of sugar or to turn a paddle wheel. The fact that such responses can be rewarded by taste stimuli permits us to say that taste stimulation is reinforcing.

Figure 11 is a graph showing the relation between the rate of bar-pressing and the concentration of two solutions, one of saccharin, the other of sucrose. The normal animal's rate increases with concentration for saccharin up to a peak value of -2 log units, but then the response rate falls with a higher concentration. This decline in response rate for the stronger saccharin solution suggests that, for the rat as for man, strong saccharin solutions may have an aversive (bitter?) taste. The curve relating rate of bar pressing to concentration of the sugar stimulus is even clearer. Such curves for sugar were first obtained by Guttman (1953) and have since been confirmed by many workers. The rate of response may be considered a measure of response vigor which is clearly influenced by the strength of the gustatory stimulus. But of particular interest is the fact that the animal does not get any more sugar from the higher rate. Bar pressing here is reinforced on a variable interval schedule, that is, the "pay-off" or reinforcement is determined by an elapsed time of variable duration, on the average, every 20 sec. The time interval between successive reinforcements is randomized so that the animal does not learn to respond at a particular time interval. Increased response rate does not increase the number of reinforcements delivered with this schedule. Thus, *it is possible to say that the sensory stimulus is a motivating stimulus, not only because it provides the reinforcement for learning the bar-press response, but because the vigor in terms of rate of response itself is determined by the strength of the sensory stimulus.*

The lower curve, with the open circles, shows the response of the desensitized animal. There is a barely detectable increase in the rate for the saccharin; the increase in rate to the sugar solution is somewhat clearer. In both cases, the rate of response is much below that of the normal animal. Nevertheless, the caloric value of the sugar for the control and operated animal is the same, only the taste efficacy has been reduced for the latter. Such evidence allows us to conclude that the taste stimulus reinforces behavior *by virtue of its direct sensory effect.*

The Specific Hunger for NaCl

Other behavior dependent on taste stimulation is that of the "specific hungers." Naturalistic observations are replete with examples of the craving for salt shown by animals in the wild. Herbivorous animals are known to travel great distances in search of a salt lick. Certain mineral deficiencies in forage which grows in depleted soil may lead to other aberrations of taste preference. The hunger for sodium chloride after deprivation is probably one of the best known and most widely studied. The

condition may result from imbalance in the hormones which regulate the salt level of the body or forced sweating and loss of sodium chloride in very humid and hot climates coupled with an inadequate intake of salt.

Wilkins and Richter (1940), at the Johns Hopkins Hospital, gave a dramatic account of hormonal imbalance and salt craving in the case of a boy who suffered from precocious sexual development, excessive hirsutism, and other signs of hypergonadism. He succumbed at the end of a week after admission to the hospital for observation. Postmortem examination revealed the presence of a tumor of the adrenal gland. Follow-up studies revealed that the boy had developed an unusual salt craving prior to hospitalization to such a degree that he was observed to empty

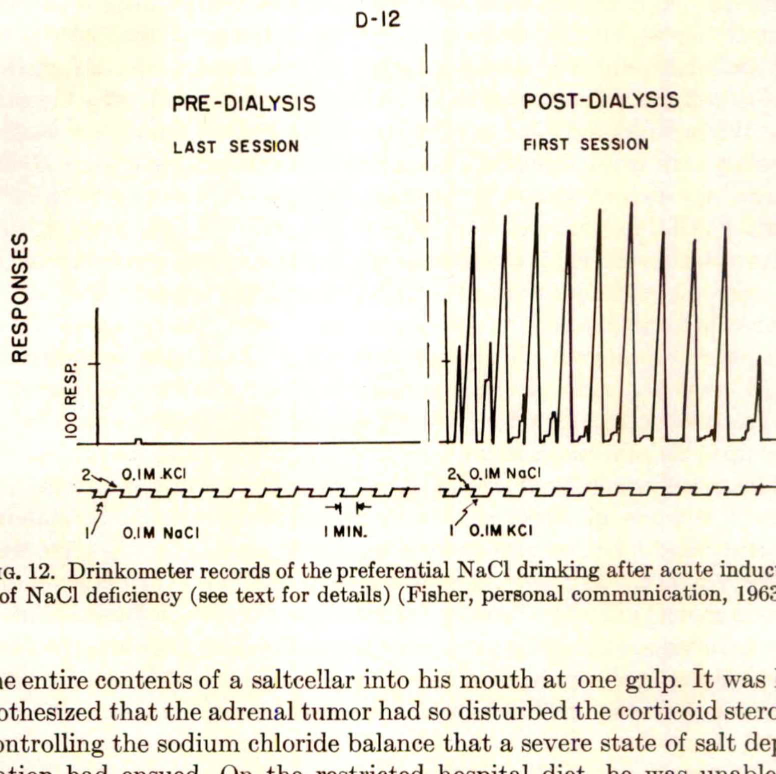

FIG. 12. Drinkometer records of the preferential NaCl drinking after acute induction of NaCl deficiency (see text for details) (Fisher, personal communication, 1963).

the entire contents of a saltcellar into his mouth at one gulp. It was hypothesized that the adrenal tumor had so disturbed the corticoid steroids controlling the sodium chloride balance that a severe state of salt deprivation had ensued. On the restricted hospital diet, he was unable to satisfy the craving and succumbed to the lack of sodium chloride.

Richter (1939; Richter and Eckert, 1938) duplicated the salt deficiency syndrome in rats by surgically removing the adrenal glands. Such an animal will succumb in a few days unless the diet is fortified with extra sodium chloride. Moreover, if two bottles are placed on the front of the animal's living cage, one containing water and the other NaCl solution, the adrenalectomized rat will immediately prefer the sodium chloride

solution, taking it in quantity sufficient to maintain good health for many months. Such animals, when desensitized by surgically removing the taste nerves, cannot make the necessary discrimination and fail to survive.

The hunger is specific for sodium chloride. Thus, the adrenalectomized rat shows the enhanced preference for NaCl or other sodium salts but not Ca, Mg, or other nonsodium salts. When offered a choice between NaCl and KCl, the salt-deprived animal almost immediately displays the differential NaCl preference. Figure 12 illustrates a procedure developed by Dr. G. L. Fisher in our laboratory. The record shows a drinkometer recording of the number of licks at each of two drinking tubes, one containing NaCl, the other KCl, when presented alternately in the testing compartment. Each lick at either drinking tube moves a cumulative recorder upward across the recording paper. At the end of a minute, one tube is automatically withdrawn and the other inserted as the recorder pen resets to baseline. The marker at the base of the record indicates the solution being presented. The control record to the left shows the relative absence of response under the normal conditions. On the right is shown the greatly increased drinking at the same time of day but shortly after acute NaCl depletion was induced by intraperitoneal dialysis with 5% glucose. Coupled with the greater intake of NaCl solution throughout the test period is a progressive decline in KCl intake. Here is graphic evidence of the Na^+ craving.

Another example of the specificity of the sodium chloride preference was shown in a recent ingenious experiment by Dr. Marvin Nachman (1962, 1963) while a visiting investigator at Brown. Rats select either lithium or sodium chlorides with equal avidity in brief (10 minute) drinking tests. Lithium chloride, however, is toxic so that the animals ingesting it soon became ill, showing obvious signs of disability and malaise. Several days later, after recovery from the toxic episode, they were retested with both lithium and sodium chloride. Now, the poisoned animals avoided not only lithium chloride but also the sodium chloride. The avoidance of potassium chloride, however, was less marked. Thus, the aversion to the salty taste of lithium chloride *generalized* most to the salty taste of sodium chloride and less to the taste of potassium and other non-sodium salts also tested.

These findings are also directly related to the work of Dr. Erickson described above on similarity of neural coding between sodium chloride and other salts. The neural discharge profiles for NaCl and LiCl are known to be highly correlated, so that, for the rat, these substances would appear to have a similar taste.

In an earlier study (Fregly, 1958), salt-needy rats showed only the preference for sodium chloride but avoidance of lithium chloride in a 24-hour two-bottle *ad lib* self-selection test. In this case, post-ingestive after-

effects during the more prolonged 24-hour test might have aided the discrimination between lithium and sodium during the 24-hour period. If the animal is allowed only a brief taste, as in the Nachman procedure, he confuses lithium and sodium chlorides.

We know that the sense of taste plays an important role in the selection of the specific substance in states of need, but there is considerable question as to whether physiological changes occur in the sensory input, at motor output pathways, or at some more central locus.

Adrenalectomized animals show a preference for weak salt solutions to which they are normally indifferent. This has been interpreted as a sign that the animal's sense of taste has become more acute so that he can detect the salt solutions in the weaker concentrations. The preference for the salt shown by the adrenalectomized rat indicates only that there is a *lowered preference* threshold but not necessarily a change in the physiological sensitivity of the end organ. Electrophysiological studies of the chorda tympani nerves in the rat indicate that the receptor threshold concentrations for normal and adrenalectomized rats are the same (Pfaffmann and Bare, 1950). More recent studies of the same kind over a wide range of suprathreshold concentrations for NaCl and with a variety of other electrolytes gave essentially the same conclusion (Nachman and Pfaffmann, 1963). The receptor itself of the salt-needy animal is physiologically unchanged. These conclusions have been confirmed for the rat by means of the conditioned reflex method (Carr, 1952; Harriman and MacLeod, 1953).

But, a recent study has reported change in taste thresholds for salt-depleted human subjects (Yensen, 1959). In two subjects, forced sweating and low salt diet significantly lowered the threshold for the recognition of salt but not for other taste stimuli during the period of salt deprivation. Henkin and Solomon (1962) also have reported enhanced sensitivity for the detection of salt in untreated patients with adrenal gland disorder with characteristically low blood serum sodium. In this case, the fall in threshold (increased sensitivity) was not specific to the salty taste; other taste substances tested yielded lower thresholds.

Mere elevation of the NaCl serum level by electrolyte-sparing corticoids did not raise the thresholds. The thresholds returned to normal only when patients were treated with gluco-corticoids. One normal subject was also tested in whom blood serum Na levels were substantially lowered with no accompanying change in taste threshold. De Wardener and Herxheimer (1957) also found no direct correlation between serum sodium level and threshold when electrolyte metabolism was disturbed by forced draughts of distilled water. They found a drop in threshold for NaCl for only part of the test period. In recent tests in our laboratory, McCutcheon (1963) failed to find a drop in threshold with fall in serum sodium level. Thus, the question of human taste threshold changes dur-

ing serum level changes requires further study to reconcile these diverse reports.

In recent experiments, McBurney and Pfaffmann (1963) showed that the sense of taste for salt is remarkably influenced by the ambient sodium chloride concentration bathing the tongue. That is, the salivary sodium may, in fact, be an agent which adapts the taste receptors in the normal observer such that the threshold of the taste buds can be lowered by at least a hundredfold by a continuous distilled water rinse between threshold determinations. Thus, changes in salivary sodium level might affect the threshold for sodium chloride.

It is also possible that changed thresholds for salt solutions could reflect excitability change in the nervous pathways through which the gustatory information is processed. It is well known that the normal sensory systems can be influenced by inhibitory influence by way of the reticular formation and other sensory efferent systems (Galambos, 1956; Hernández-Peón, 1955), although such an effect has not been identified physiologically for taste.

Thus, although the sense of taste does play a significant role in the specific cravings and hungers, there is still need for further investigation as to the precise physiological mechanism involved. It is also important to note that a simple sensory threshold change by itself will not adequately account for the changed preference seen at all levels of salt concentration. Salt-deprived animals show an enhanced craving for the strong as well as the weak salts. Normally, strong salts are avoided and taken only in small amount. The deprived animal increases the intake of the normally aversive as well as normally acceptable stimuli. Thus, there is a change in the motivational value of both strong and weak sensory stimuli which cannot be explained by a purely sensory change.

Summary

I have described at some length the electrophysiological method which permits the study of the afferent code in sensory nerves consequent upon taste stimulation. This sensory code is somewhat more complex than was originally proposed in the classical theory of "four primary tastes." Actually, the sensory input for taste might be described as a "neural profile." Although some sensory neural units may be specific to one class of chemicals, others respond to several primary tastes or even a wide range or "spectrum" of tastes. The neural discharge in any one sensory channel would have a different meaning depending upon the concurrent activity in other parallel sensory channels at the same time. The sensory code appears to be reflected in the ratios of frequencies simultaneously present in a number of taste fibers.

I then gave an account of two aspects of the behavioral responses to taste stimulation. One, the so-called "sweet tooth," illustrates the fact

that certain innately positive stimuli not only instigate ingestion of food but also could serve as rewards or reinforcement for learning. Almost any instrumental response could be learned on this basis. In addition, the vigor of the response as reflected in the rate of response appears to be determined by the strength of the taste stimulus reinforcing that response.

Finally, the remarkable specificity of the "specific hunger" for NaCl was described. Although this phenomenon has been intensively studied, there still remain many questions as to the precise physiological mechanism by which such specific hungers are guided and regulated. Physiological studies suggest that any changes in the sensory reaction to stimulus lies not in the receptor itself but perhaps more centrally in the brain.

The general conclusion can be drawn that the sense of taste has two major features. One provides information concerning the quality and quantity of diverse chemical stimuli on the tongue. Secondly, this information provides motivation for the control of behavior. Such motivation, whether positive or negative, appears to result directly from sensory stimulation itself without the necessity of prior conditioning or learning.

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AMERICAN SCIENTIST, *52*, 1964

ELECTRIC PROPULSION

By ROBERT G. JAHN

A ROCKET in free flight accelerates itself by the discharge of propellant mass in a direction opposite to the desired acceleration. Its equation of motion thus follows directly from a conservation of momentum of the complete system—rocket and exhaust stream:

$$m \frac{dv}{dt} = v_e \frac{dm}{dt} \tag{1}$$

where m is the total mass of the rocket at any given time, including its casing, engine, payload, and unused propellant; dv/dt is its acceleration; v_e is the velocity with which the exhaust jet leaves the rocket, relative to it; and dm/dt is the rate at which propellant mass is used in this fashion. The product $v_e(dm/dt)$ is usually called the thrust of the rocket. If one assumes a constant exhaust speed, v_e, the velocity increment, Δv, which the rocket can achieve by discharging some portion of its propellant mass, Δm, follows from simple integration:

$$\Delta v = v_e \ln_e \left(\frac{m_0}{m_0 - \Delta m} \right) \tag{2}$$

where m_0 is the total mass of the rocket at the start of the acceleration. By expending all of its propellant mass in this way, the rocket can achieve a maximum velocity increment

$$\widehat{\Delta v} = v_e \ln_e \frac{m_0}{m_f} \tag{3}$$

where m_f is the initial rocket mass, less all the propellant, i.e., the payload, engine, and casing. Conversely, the fraction of original rocket mass which can be accelerated through a given velocity increment, Δv, i.e., the deliverable mass fraction, is a negative exponential in the ratio of Δv to the exhaust velocity:

$$\frac{m_f}{m_0} = e^{-(\Delta v/v_e)} \tag{4}$$

It can be shown that the more complicated rocket missions of practical interest, involving flight through various planetary, lunar, and solar gravitational fields, with variable magnitude and direction thrust programs, can satisfactorily be represented by characteristic velocity increments, Δv, each satisfying relation (4) for the particular mission involved. In general, long-range missions, or missions in strong gravitational fields require large Δv, and vice versa. For example, a round trip from a suitable parking orbit around the earth to a similar orbit about Mars or Venus

would be characterized by a Δv of about 25,000 m/sec. A similar excursion to Jupiter requires Δv of about 75,000 m/sec.

For such long-range missions, then, a premium must be placed on the generation of comparably high propellant exhaust velocities, v_e, if significant fractions of the original rocket mass are to be delivered to the destinations. Unfortunately, the exhaust velocities attainable by nozzled expansion of the high-temperature products of a combustion reaction, as in the conventional chemical rockets, are fundamentally limited by the available reaction energies to about 4000 m/sec, with possible extension to 6000 m/sec with certain highly exotic fuel-oxidizer combinations. Heating a propellant gas within a solid core nuclear reactor circumvents the chemical limitations, and holds promises of exhaust velocities ap-

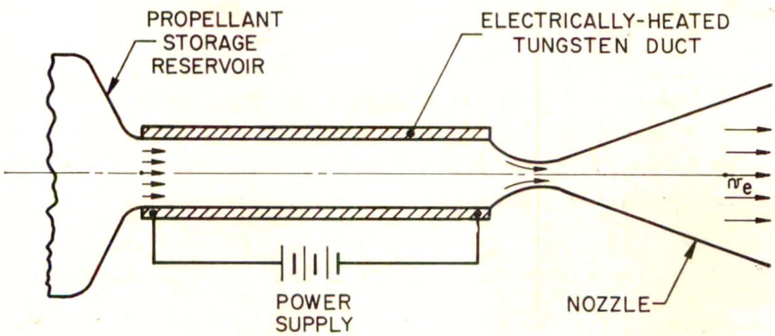

FIG. 1. The resistojet (schematic).

proaching 8000 m/sec before heat transfer problems threaten structural integrity of the engine. These two rather basic limitations—available chemical energies, and heat transfer tolerances—predicate study of alternative means of accelerating gases to exhaust velocities commensurate with the characteristic velocity requirements of the longer missions.

Electric propulsion comprises three rather distinct endeavors in this direction:

(1) Electrothermal Propulsion: heating of a propellant gas by electrical means for subsequent expansion through a nozzle.
(2) Electrostatic Propulsion: direct acceleration of ionized gas particles by electrostatic forces.
(3) Electromagnetic Propulsion: direct acceleration of an ionized gas by the simultaneous application of electric and magnetic body forces.

(1) The concept of electrothermal propulsion clearly does not eliminate the heat transfer process; rather, it attempts to exploit it. Two distinct efforts are underway in this field, a rather modest study of the "resisto-

jet," and a much larger and more vigorous set of programs on arcjet thrustors. The resistojet simply invokes an electrically heated solid

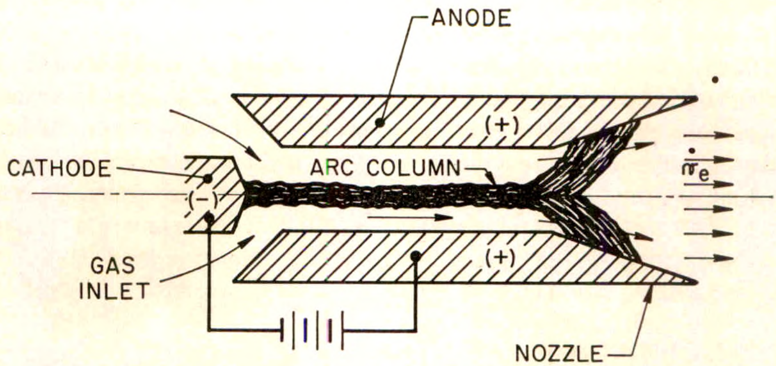

FIG. 2. The arcjet (schematic).

element, such as a coil of resistance wire immersed in the gas flow, or better, a refractory duct through which the flow passes, to heat the propellant gas prior to its expansion through the nozzle (Fig. 1). Since there

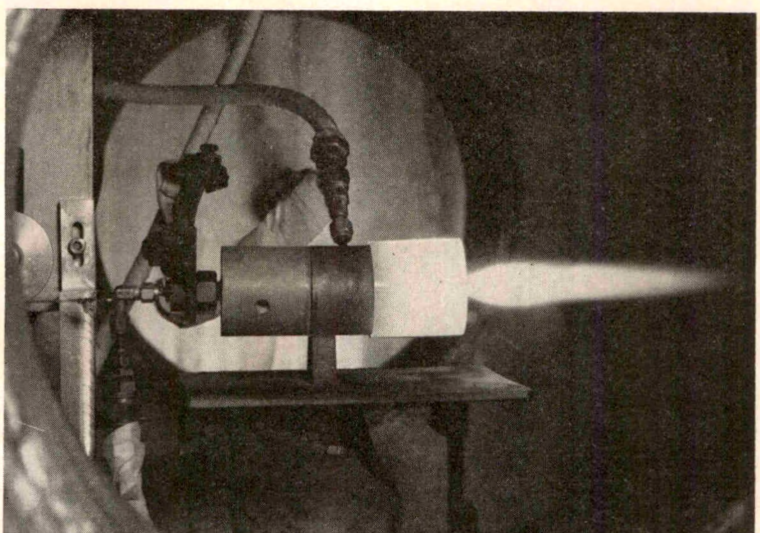

FIG. 3. Thirty kilowatt arcjet engine in laboratory operation. (*Courtesy AVCO Research and Advanced Development Division*)

is a freedom of choice of propellant gas, and good control over the heating program along the duct, it is possible to raise the gas temperatures nearly to the softening point of the best refractory solid materials, say 3000° K, with a resulting exhaust velocity approaching 10,000 m/sec,

while yet restricting engine erosion to tolerable levels for the long missions contemplated. The simplicity of this device is attractive, but it is doubtful that its exhaust velocity will ever substantially exceed the 9000 m/sec level it has already demonstrated [1].

The arcjet engine heats the propellant gas by a flow-stabilized, high-current electric arc. Steady currents of hundreds, or even thousands of amperes are passed through the gas between a tungsten button cathode and an annular anode, generating a tightly constricted arc column reaching temperatures of tens of thousands of degrees Kelvin on its axis.

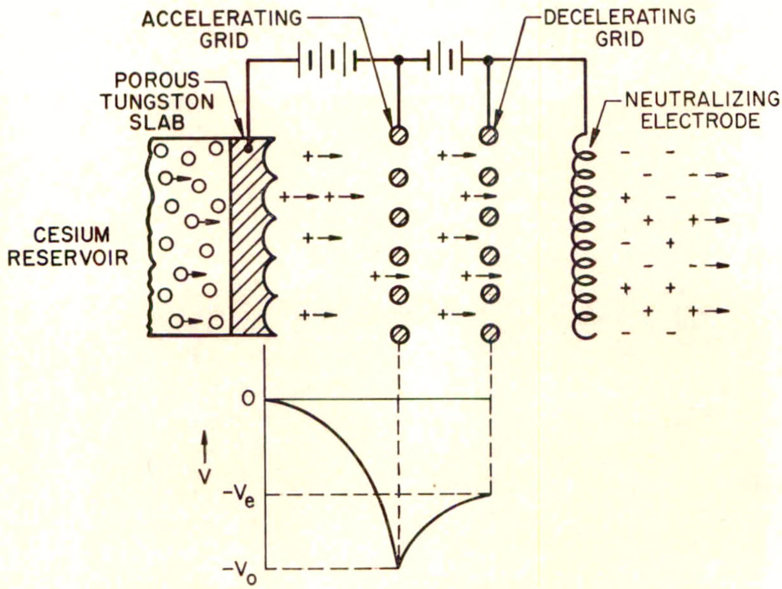

FIG. 4. Cesium ion engine (schematic).

Incoming gas flow swirls around and through this arc, is heated by it, and expands through a nozzle to velocities of 20,000 m/sec or higher (Fig. 2). The device owes its practicability to the steep radial temperature profile prevailing in the arc chamber and persisting far into the nozzle, which permits the central flow to reach very high temperatures, while the flow near the walls remains cool enough that erosion is kept to a tolerable level. Some 30 kw space engines of this type, generating 1.5 newtons thrust (about ½ pound) have been operated successfully for hundreds of hours in the laboratory, (Fig. 3) and may already have been space tested by the time of publication of this article [2].

(2) In electrostatic propulsion, one completely takes leave of conventional rocket components, including heating chambers, nozzles, and even continuum gas flows, and attempts to accelerate individual charged

particles in electrostatic fields established by electrode structures like those employed in thermionic vacuum tubes and cathode ray oscilloscopes. In one popular class of ion engine, an alkali vapor of low ionization potential, such as cesium, is forced through a hot porous tungsten block, wherein most of the vapor atoms lose one electron and emerge as positively charged ions (Fig. 4). These are accelerated through a large voltage drop, V_0, by a grid electrode, thus acquiring velocities determined by their charge to mass ratio, q/m_+:

$$v_e = [2V_0q/m_+]^{1/2}$$

Subsequent to this acceleration, it is necessary to reintroduce into the ion stream the electrons previously left behind on the tungsten source,

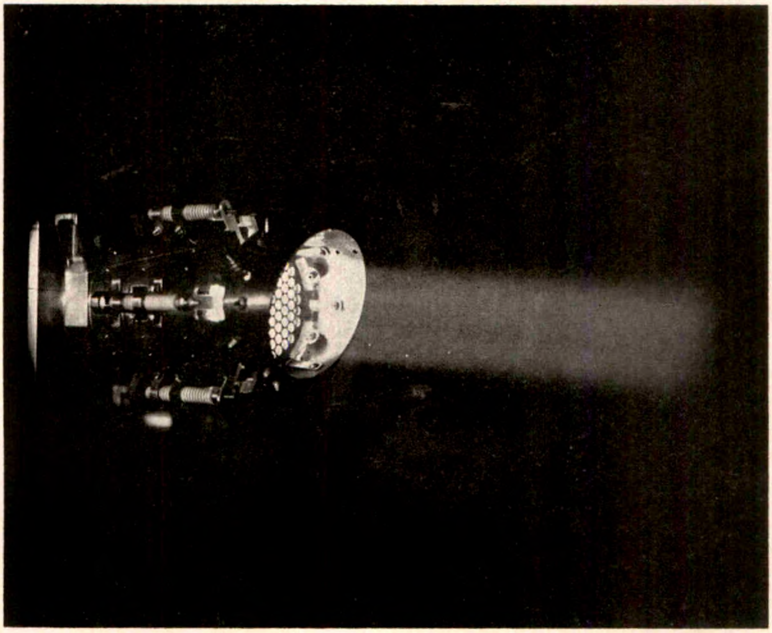

FIG. 5. Flight-test model of cesium contact ion engine in laboratory operation. (*Courtesy of Electro-Optical Systems, Inc.*)

lest the exhaust beam stall itself by its own net space charge field. This charge neutralization is commonly accomplished by a thermionic electron-emitting element immersed in the exit ion beam. To inhibit the neutralizing electrons from accelerating back toward the electrode gap, a second grid at some intermediate potential, $-V_e$ is frequently inserted between the accelerating grid and the neutralizing electrode. The ion beam is thereby decelerated somewhat, and emerges with a velocity now determined by $\sqrt{V_e}$.

Many geometries of such cesium ion engines exist (Fig. 5), as well as others employing mercury ions prepared by electron bombardment (Fig. 6), heavy colloid ions, and even charged oil droplets. In all of these, the limitation is not so much on attainable exhaust velocity, which is restricted only by the voltage which can be maintained on the electrodes, but rather on the total thrust levels which can be generated on this particle flow basis. It is a relatively straightforward task to build an ion engine with exhaust velocities greater than 100,000 m/sec; its thrust,

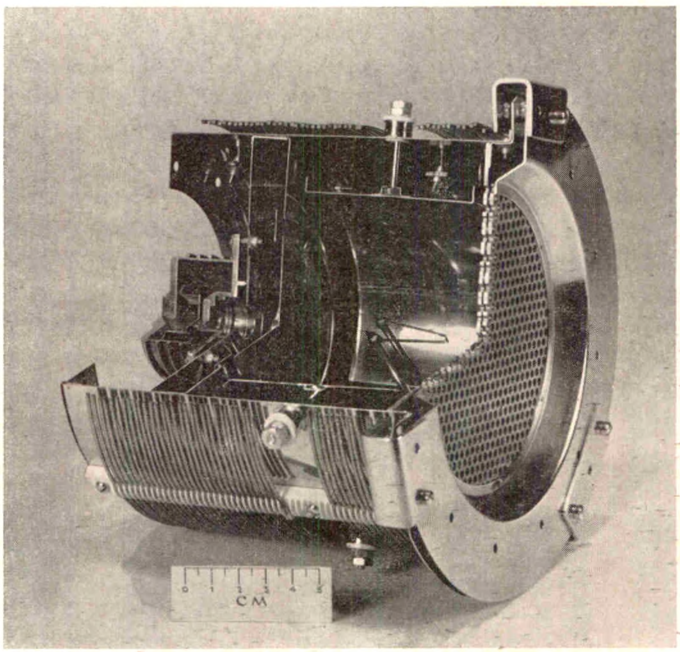

FIG. 6. Cutaway view of electron-bombardment ion engine. *(Courtesy of NASA Lewis Research Center)*

however, at the current state of the art, would probably not exceed 0.1 newton. At present, workers in this field would gladly trade a large fraction of the exhaust velocity capability for more thrust, and the ion engine designs reflect this attitude. For the very distant space missions, of course, high exhaust velocities will be indispensable, and for these the ion engine seems to be a leading possibility [3].

(3) Electromagnetic propulsion comprises a variety of schemes for the acceleration of gas flows by the application of magnetic body forces. In principle, these promise exhaust velocities in the range from 20,000 to 100,000 m/sec with thrust levels substantially higher than pure electrostatic devices. In practice, the complexity of the phenomena involved

have so far largely restrained this effort to basic research, although a few empirical thrustors have been constructed.

The electromagnetic accelerators should logically be subdivided into steady flow, and pulsed classes. In the former, a steady stream of previously ionized gas is caused to flow between two electrodes, which drive a current through it. An externally applied magnetic field, perpendicular both to the flow and to the transverse current, then exerts a streamwise force on the current-carrying electrons within the gas, which in turn transmit it to the bulk of the gas (Fig. 7). Such devices have already demonstrated thrusts of tens of newtons [4], but involve massive appen-

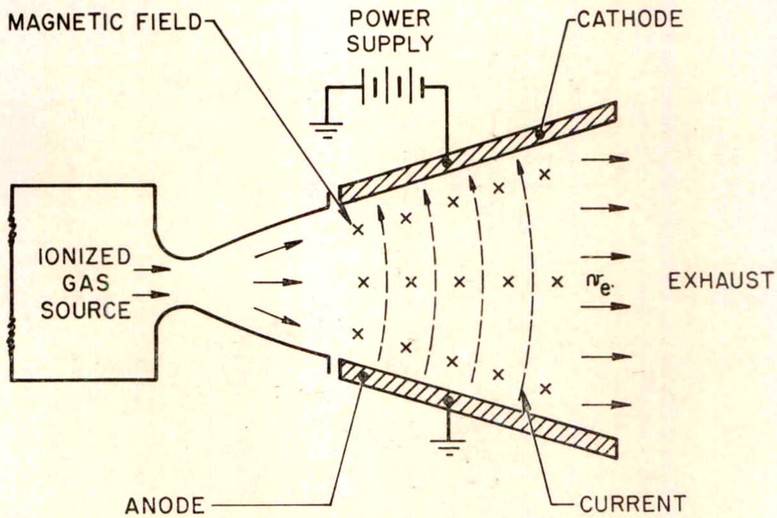

FIG. 7. Crossed-field accelerator (schematic).

dages of auxiliary equipment, such as the field magnets, gas ionizers, and electrode cooling gear. Their practicality for space propulsion may hinge on the development of techniques for increasing the ionization level of the propellant gas above that normally prevailing at the maximum tolerable temperatures, and on the development of very low specific mass magnets, such as the cryogenic super-conducting devices now under laboratory study, in a form suitable for space flight.

The pre-ionizing equipment and the heavy field magnets can be eliminated if the electrodes are able to apply a very high voltage to the gas, and then are able to sustain a very high current through it. Several kilovolts suffice to break down an unionized gas at useful density levels, and discharge currents in the hundred thousand ampere range generate their own magnetic fields in sufficient strength to accomplish the desired acceleration. However, such power levels in space could reasonably be

sustained only in pulses, derived from some electrical storage unit, such as a capacitor bank. Thrustors operating in this manner comprise the pulsed plasma propulsion engines of which many geometries have been explored. At least one of them, the "pinch" engine, is currently ready for space testing [5] (Figs. 8, 9). While eliminating the need for the magnet and pre-ionizer, these engines assume the new burden of the capacitor banks, and suffer an inherent performance penalty by thrusting only in discrete pulses. The present effort in this field is toward increasing the repetition rate of the high current discharges, and toward improving the efficiency of the discharges in sweeping relatively large masses of neutral gas ahead of them.

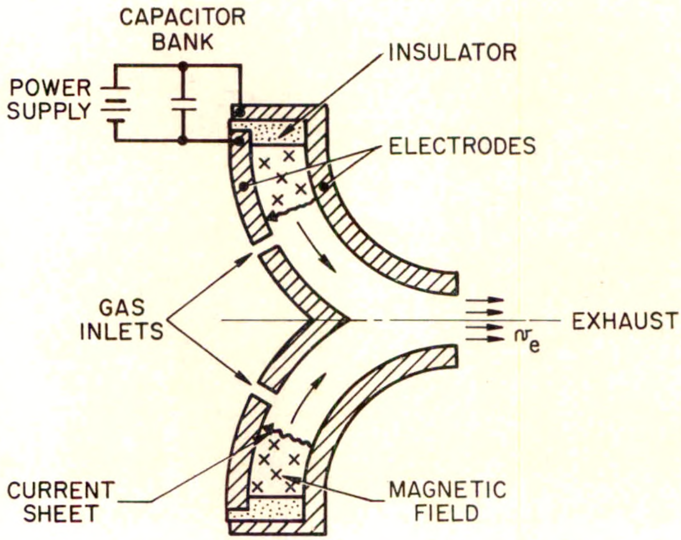

FIG. 8. The pinch engine (schematic).

A hybrid type of electromagnetic thrustor of some interest has evolved from the traveling wave accelerators of elementary particle physics. In this device, a suitably programmed wave guide or transmission line generates a traveling electromagnetic wave pattern which propagates with an appropriate velocity profile along the interior of a duct. Ionized gas introduced at the duct entrance tends to agglomerate in a succession of blobs at one phase of the propagating wave pattern and is thus convected in "surf-board" fashion down the duct by the traveling wave. The exhaust velocity attainable in principle is quite high, but the efficient coupling of the wave pattern to plasma blobs of usefully high density has so far proved troublesome [6] (Fig. 10).

As the electric propulsion field has developed, the empirical subdivision

into electrothermal, electrostatic and electromagnetic devices has tended to soften, with the realization that in many cases two or even all three of the mechanisms contribute to the operation of a single thrustor. Some devices, for example, invoke magnetic forces for containment of the plasmas, and electric forces for the actual acceleration. Likewise, a few electrothermal accelerators benefit from a magnetic confinement of the hot gas flows—away from their material walls. In a very recent and important development, it has been shown that certain arcjets, when oper-

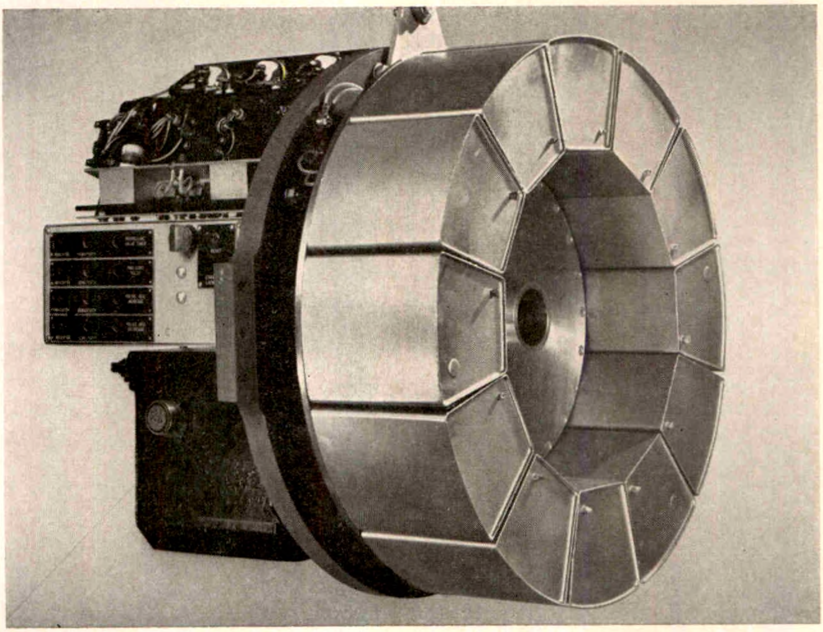

FIG. 9. One kilowatt pinch engine system for space test—including batteries, converters, propellant handling system, capacitor bank, and thrustor. (*Courtesy of Republic Aviation Corp.*)

ated at low chamber density and high current levels become predominantly electromagnetic, rather than electrothermal devices, and display correspondingly higher exhaust velocities.

At this time, it is unreasonable to establish any order of preference among the various electric thrustors described above. Each operates over a particular range of exhaust velocity, and each has characteristic advantages and weaknesses, most of which have not yet been adequately explored. In view of the variety of long range space missions which will be desired in coming years, it would seem wise to continue a balanced research program covering all of these areas, for the purpose of ascertaining and developing the best engines for specific tasks.

The need for optimization of the thrustors in terms of specific missions is particularly critical from the standpoint of the electric power supplies needed to drive them. The power consumed by a given thrustor varies as the square of its exhaust velocity, for fixed mass flow. The mass of a space nuclear power supply, intrinsically large because of the bulky reactor and generator elements involved and because of the large radiator surfaces needed to dispose of waste heat in space, clearly increases with

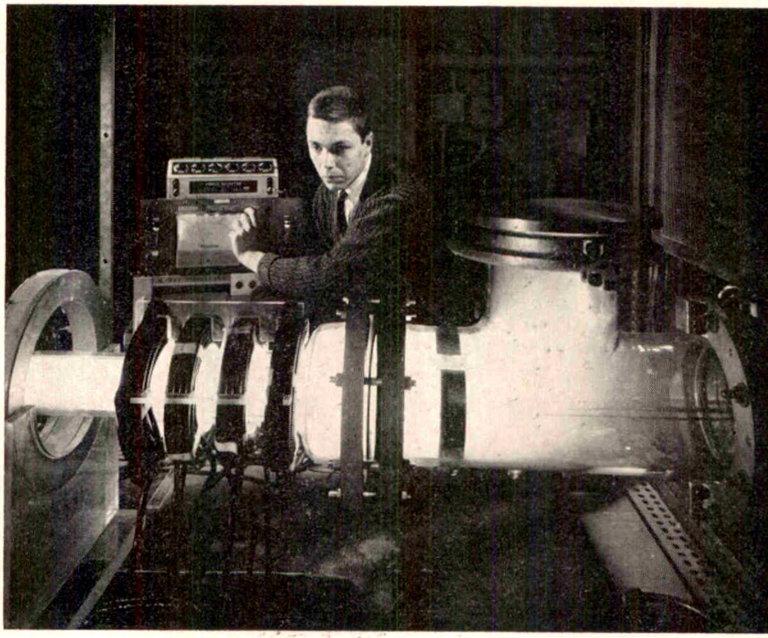

FIG. 10. Traveling wave accelerator in laboratory operation. *(Courtesy of NASA Lewis Research Center)*

its power capacity. It follows, then, that there is an optimum exhaust velocity for any mission; operation at too low a value reduces deliverable payload mass ratio, as we have seen, because excessive propellant mass is required; but operation at too high an exhaust velocity requires an excessively massive power supply and thereby also reduces the payload ratio.

On this basis there is adequate incentive for the development of an assortment of electric thrustors, consisting, say, of electrothermal devices for the 10,000–25,000 m/sec range, electromagnetic engines for the 20,000–100,000 m/sec regime, and ion engines for 50,000–200,000 m/sec and beyond. Such a spectrum of electric thrustors, coupled to the appropriate power supplies, would then constitute a serviceable arsenal for the attack on interplanetary travel.

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AMERICAN SCIENTIST, *52*, 1964

THE COMPUTER VERSUS KEPLER*

BY OWEN GINGERICH

WE OFTEN hear, in discussions of modern high-speed computers, how an electronic machine can calculate more in a day than a man can calculate in a lifetime by older methods. It occurred to me that a concrete demonstration of some properly chosen specific case would not only be intrinsically interesting, but might shed some light on the historical situation in question, and might also provide a dramatic example of the application of computers in the history of science.

An especially appropriate example is found in the work of Kepler on the orbit of Mars, since he gives some indication of the computational time involved. In *Astronomia Nova,* Kepler describes in detail his attempt to fit a circular orbit to a series of observations of Mars at opposition. Since he wished to investigate a somewhat more general orbit than had been adopted classically, he was led to a thorny trigonometric problem that can be solved only iteratively.

Concerning this involved procedure, Kepler implores his reader: "If you are wearied by this tedious method, take pity on me, who carried out at least seventy trials of it, with the loss of much time, and don't be surprised that this already is the fifth year since I have attacked Mars, although the year 1603 was almost entirely spent on optical investigations."[1] [1].

The implication that this problem required four years must be taken with a grain of salt, but we do get a rough idea of the time involved.

It is this tedious, time-consuming procedure that I have programmed for the IBM-7094 at the Harvard Computing Center. Before describing my quite unexpected results, let me outline Kepler's problem in somewhat greater detail.

When Kepler started his investigation on the motion of Mars, in 1601, he was already a convinced Copernican, and therefore he assumed a heliostatic orbit. Nevertheless, at the beginning, he accepted the classical idea of using circles to represent the motion, and not until two years later did he work out the elliptical form of the orbit. The "vicarious orbit" that caused Kepler so much anguish and loss of time was a circle, and in the end was completely abandoned.

Kepler had in hand a dozen observations of Mars at opposition—

* Presented to the History of Science Society, Philadelphia, December 29, 1963.

[1] To this, the French astronomer Delambre replied: "Kepler was sustained by his desire to have a case against Tycho, Copernicus, Ptolemy, and all the astronomers in the world; he has tasted this satisfaction, and I don't believe he deserves our pity for making all these calculations [2].

ten from Tycho Brahe and, later, two of his own [3]. When Mars is at opposition, the sun, earth, and Mars lie in a straight line, so the heliocentric longitude of Mars is immediately known. Figure 1, reproduced from Delambre's *Histoire de l'Astronomie Moderne,* shows us the basic diagram for this problem. In the diagram, the sun is at A, and four observations of Mars, carefully chosen for a reasonably uniform distribution, are laid out from it. Note that the earth does not enter into this discussion. Now the correct elliptical orbit of Mars does not differ very

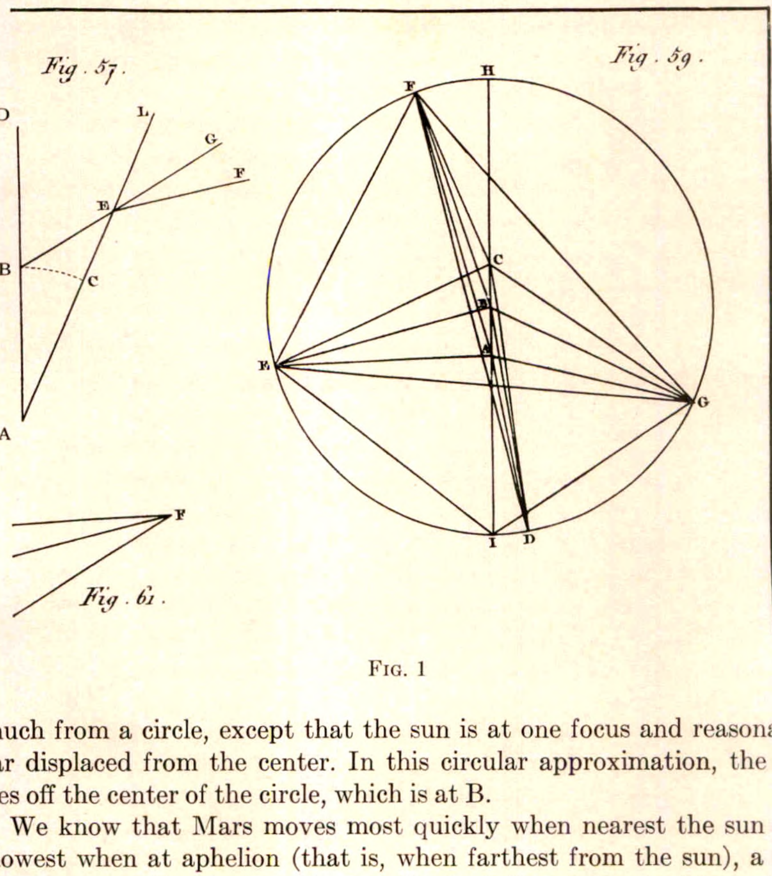

FIG. 1

much from a circle, except that the sun is at one focus and reasonably far displaced from the center. In this circular approximation, the sun lies off the center of the circle, which is at B.

We know that Mars moves most quickly when nearest the sun and slowest when at aphelion (that is, when farthest from the sun), a fact later expressed in the law of areas. Kepler believed this must be so from physical reasons, and therefore, he was already convinced that the seat of uniform angular motion in the orbit, must lie on the line through A and B, that is, on the line of apsides. In the analogous case, Ptolemy had placed this seat of uniform angular motion, or equant, equally spaced opposite A from the center of the circle. We now know that such a configuration produces the best possible approximation to an ellipse, and when we have the equant at the empty focus of the ellipse,

the resulting errors in fitting the observed longitudes reach a maximum of 8′ of arc. This is the figure later found by Kepler, which, for him, proved to be such a large discrepancy from Tycho's observations that he felt obliged to abandon the circular orbits.

Kepler, however, wished to keep the spacing of A and C along the line of apsides as an unknown quantity to be determined. Also, he knew the direction of the aphelion fairly well, but he wished to improve its position. Kepler was therefore obliged to use four observations to determine all these quantities. Nowadays, we would try to use all twelve observations, combining them into a least-squares solution. This technique was, of

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$\left(\frac{AG + AE}{AG - AE}\right)$ inverted $=$ tan. 19° 57′ 40″ $\left(\frac{103021}{1544}\right)$ = tan. 16′ 11″;
and AEG = 19° 55′ 51″; as also EG = $\frac{\text{AG. sin. EAG}}{\text{sin. AEG}} = \frac{52282.63271}{54088} = 97041.$

3. Since the base EG of the isosceles triangle EBG, and the vertical angle EBG are thus found, the angle BEG at the base is given, and = 25° 46′ 53″; and, therefore, BE = $\frac{\text{EG. sin. BEG}}{\text{sin. EBG}} = \frac{97041.43494}{78327} = 53860.$

4. In the triangle BEA, the angle BEA is given, for it is = BEG − AEG = 5° 51′ 2″, and also its ½ suppl. = 87° 4′ 27″. Therefore tan. ½ (BAE − ABE) = tan. ½ suppl. BEA $\left(\frac{BE - AE}{BE + AE}\right) = \frac{1957200.3121}{104599} = 58402$ = tan. 30° 17′ 8″; so that BAE = 117° 21′ 37″.

But, since in the second operation the aphelion H was found to be too far advanced in longitude, let it now, in consequence of the last correction, be considered as advanced no more than 3′ 8″, instead of 3′ 20″ beyond the longitude first assumed. Then, since AH is in 4s. 28° 47′ 8″, and AE in 8s. 26° 39′ 23″, CAE or HAE will be = 117° 52′ 5″; that is, greater by 30′ 28″ than BAE; and B is not situated in AC, but on side of it towards E. The suppositions therefore for FAH and FCH, must, one of them, or perhaps both, be false.

But these angles of anomaly cannot be varied by the mere variation of the assumed longitude of the aphelion; because no other position of it will permit the points D, F, F, G, to be situated in the circumference of the same circle; and before it can be farther varied, the mean longitudes, or the position of the lines FC, FE, &c. must be varied. This, therefore, was the next step of Kepler's procedure; and he tells us, it was not till after a great variety of unsuccessful trials, that he found his purpose would be nearly accomplished by the addition of 2′ more to the longitude of the aphelion, and of 30″ at the same time to the mean longitudes. By these additions the mean anomalies FCH, ECH, &c. are all diminished 1′ 30″ each; and we have FCH = 32° 3′ 36″; KCE = 53° 7′ 2″; KCD = 11° 0′ 44″; and KCG = 68° 18′ 1″. The angles again of equation will become AFC = 5° 8′ 26″; AEC = 9° 4′ 41″; ADC = 2° 17′ 10″; and AGC = 10° 19′ 45″; being increased 30″ in the first semi circle of anomaly, and as much diminished in the second: consequently, the

```
 1000 CONTINUE
      N1CNT=N1CNT+1
      WRITE OUTPUT TAPE 6,101,N1CNT,N2CNT,ADDS,BCDATE,(CHS,N=1,4),BMEAN,
     WANCM,BAPP,EUN,(AF(N),N=1,4),TAN2,SUM1S,SUM2S
      GO TO K1,(220,230)
C-----ACC ARBITRARY INCREMENT IN FIRST ITERATION.
  220 ACC=RACF(0.,0.,5.,0.01)
      ASSIGN 230 TO K1
  225 SM1=SUM1
      SM2=SUM2
      SC=SUMC
      CHCLD=CH
      CH=CH+ACC
      GO TO 190
C-----ACC PROPORTIONAL INCREMENTS IN REMAINING ITERATIONS.
  230 IF(ACC-RACF(C.,0.,0.,10.))235,235,234
  234 ACC=ACC/(SC-SUMC)*SUMC
      IF(N1CNT-20) 2 5,300,300
C
C-----BEGIN OUTER ITERATION.
  235 EBG= TWOPI-FAF-FAE(4) -SUM1-SUM2
      EAG=FAE(2)+FAF(3)
      AEGAGE=2.*ATANF(TNHSUPF(EAG)*ABAHF(AF(4),AF(2)) )
      AEG=(PI+AEGAGE-EAG)/2.
      EG=AF(4)*SINF(EAG)/SINF(AEG)
      BEG=(PI-EBG)/2.
      BE=EG*SINF(BEG)/SINF(EBG)
      CA=1./BE
      BEA=BEG-AEG
      BAEABE=2.*ATANF(TNHSUPF(BEA)*ABABF(BE,AF(2))  )
      BAE=(PI+BAEABE-BEA)/2.
```

FIG. 2. The comparison of the Robert Small commentary with a portion of the FORTRAN program shows how closely the notations agree.

course, unavailable to Kepler. Note that the angles from A are all determined by observation. The angles from C are known relative to one another, because the motion about this point is uniform in time and the times of observations are known. The zero point of this system is to be determined, and also the direction of the aphelion AH.

Kepler starts by assuming these two quantities and solves trigonometrically the various angles of this inscribed quadrilateral. The result tells him whether or not the points lie on a circle. In the first instance they do not, so the direction AH is altered and the solution made again. A comparison of the results of these trials suggests a better position for AH, and the calculation is again repeated. This process I shall call the inner iteration. When it has finally converged, Kepler solves this triangle EGB to find if the center of circle B lies on the line CA between

the sun and the equant. Again in this first instance it does not. This time, the zero point of the mean angles at C is altered, and the inner iteration is repeated. Eventually, the outer iteration also succeeds, and the points A, B, and C are found to lie on a straight line. I am sure Kepler is counting the inner iterations when he tells us that seventy trials were required.

The programming followed Kepler's procedure almost exactly. I was greatly helped by a book by Robert Small [4], which was recently reprinted through the efforts of William Stahlman. Figure 2 shows how closely the FORTRAN programming followed his notation. The principal difference in my approach is that when Kepler got close to the solution, he jumped to the answer using small corrections made by proportional parts, whereas I found it easier simply to repeat the entire calculation. Also, the program used accuracy criteria somewhat more rigid than Kepler's.

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KEPLER'S VICARIOUS ORBIT, OR, 'THE COMPUTER VERSUS KEPLER'          12/18/63       PAGE 6

                         709/7090 FORTRAN DIAGNOSTIC PROGRAM RESULTS

          SUM1=ATANF(TAN2(4)-ATANF(TAN2(1))

03122     TOO MANY LEFT PARENTHESIS.

          SUM2= ATANF(TAN2(3)-ATANF(TAN2(2))

03122     TOO MANY LEFT PARENTHESIS.

  102     FORMAT(18H4OUTER ITERATION =I3,24X4HADM=3X,3F4.0,F5.1,8X,23HFINAL COMPARISON ANGLES/14X,4HEBG=3X,3F4.0,F5.1,
          8X,4HEAG=3X,3F4.0,F5.1, 8X,4HBAE=,3X,3F4.0,F5.1/14X,4HAEG=3X,3F4.0,F5.1,8X,4HBEG=3X,3F4.0,F5.1,8X,4HHAE=3X,3
          F4.0.F5.1/15X,3HEG=3X,F11.8,3HBE=3X,F11.8/      15X,3HBA=3X,F11.8,14X,3HCA=3X,F11.8)

04092     FORMAT STATEMENT IS INCORRECTLY WRITTEN.
                         END OF DIAGNOSTIC PROGRAM RESULTS.

    SOURCE PROGRAM ERROR.  NO COMPILATION.

EXECUTION DELETED.
```

FIG. 3. FORTRAN diagnostic. A decimal has been mispunched in place of a comma in the format statement.

After I had set up and "debugged" this program, I found that the machine could polish off the entire problem in a little less than eight seconds! This is not too surprising when we realize that only about twenty five trigonometric functions are required in each trial. Unlike Kepler, the computer does not need to look up and laboriously interpolate each of these. Instead, it computes them from scratch as needed, at the rate of 3000 per second!

At least some readers will want to know how long it took *me* to set up the program. When Kepler first arrived at Tycho's establishment, he made a bet that he would have the Mars orbit all cleaned up within eight days. When I agreed to report on this project, I too hoped to finish the calculations very quickly. But I procrastinated, and finally only eight days remained before the Christmas meeting. Thus, circumstances forced me to carry out these computations within that time span. In all, I had nine tries on the computer for this work. In the first two, the

```
N=  1   ITERATION=  1              ADD=   0. -0. -0. -0.

                          1587 MAR   6        1591 JUN   8         1593 AUG 25          1595 OCT 31
APHELION               4. 28. 44.   0.0    4. 28. 44.   0.0    4. 28. 44.   0.0    4. 28. 44.   0.0
MEAN LONGITUDE         6.  0. 47. 40.0     9.  5. 40. 19.1    11.  9. 49. 35.8     1.  7.  6. 50.3
MEAN ANOMALY           1.  2.  6. 56.0     4.  6. 56. 19.1     6. 11.  5. 35.8     8.  8. 22. 50.3
APPARENT LONGITUDE     5. 25. 43. -0.      8. 26. 39. 24.2    11. 12. 10. 31.8     1. 17. 24. 21.3
EQUATION OF CENTER     0.  5.  7. 56.0     0.  9.  4. 11.0    11. 27. 42. 20.0    11. 19. 45. 45.0
RADIUS VECTOR             5.94300860          5.07040501          4.80596131          5.23072946
TAN(HALF DIFFERENCES)    -0.07794160         -0.03456920          0.06613767          0.03087720

              SUM1     0.  6. 13. 31.0
              SUM2     0.  5. 45. 49.6

N=  2   ITERATION=  1              ADD=   0.  0.  5.  0.0

                          1587 MAR   6        1591 JUN   8         1593 AUG 25          1595 OCT 31
APHELION               4. 28. 49.   0.0    4. 28. 49.   0.0    4. 28. 49.   0.0    4. 28. 49.   0.0
MEAN LONGITUDE         6.  0. 47. 40.0     9.  5. 40. 19.1    11.  9. 49. 35.8     1.  7.  6. 50.3
MEAN ANOMALY           1.  2.  1. 56.0     4.  6. 51. 19.1     6. 11.  0. 35.8     8.  8. 17. 50.3
APPARENT LONGITUDE     5. 25. 43. -0.      8. 26. 39. 24.2    11. 12. 10. 31.8     1. 17. 24. 21.3
EQUATION OF CENTER     0.  5.  7. 56.0     0.  9.  4. 11.0    11. 27. 42. 20.0    11. 19. 45. 45.0
RADIUS VECTOR             5.92923105          5.07594460          4.77030510          5.22770876
TAN(HALF DIFFERENCES)    -0.07627323         -0.04007670          0.07149462          0.03045668

              SUM1     0.  6.  6. 22.3
              SUM2     0.  6. 23.  3.8

N=  3   ITERATION=  1              ADD=  -0.  0.  1. 52.8

                          1587 MAR   6        1591 JUN   8         1593 AUG 25          1595 OCT 31
APHELION               4. 28. 47.   7.2    4. 28. 47.   7.2    4. 28. 47.   7.2    4. 28. 47.   7.2
MEAN LONGITUDE         6.  0. 47. 40.0     9.  5. 40. 19.1    11.  9. 49. 35.8     1.  7.  6. 50.3
MEAN ANOMALY           1.  2.  3. 48.8     4.  6. 53. 12.0     6. 11.  2. 28.6     8.  8. 19. 43.1
APPARENT LONGITUDE     5. 25. 43. -0.      8. 26. 39. 24.2    11. 12. 10. 31.8     1. 17. 24. 21.3
EQUATION OF CENTER     0.  5.  7. 56.0     0.  9.  4. 11.0    11. 27. 42. 20.0    11. 19. 45. 45.0
RADIUS VECTOR             5.93441421          5.07386243          4.78371644          5.22884607
TAN(HALF DIFFERENCES)    -0.07690100          0.03800140          0.06947503          0.03061499

              SUM1     0.  6.  9.  3.6
              SUM2     0.  6.  9.  1.8

OUTER ITERATION =  1                  ADM=     0. -0. -0. -0.            FINAL COMPARISON ANGLES
            ELG=    4.  8. 26. 51.6   EAG=     4. 20. 44. 57.1           BAE=     3. 27. 34. 19.1
            AFG=    0. 19. 55. 57.8   HEG=     0. 25. 46. 34.2           HAE=     3. 27. 52. 17.0
             EG=    9.70434964         BE=    5.38831323
             PA=    0.11485431         CA=    0.18558683
```

FIG. 4. Intermediate computer printout. The inner iterations are carried out until SUM1 and SUM2 agree; in the outer iteration angles BAE and HAE are compared, and the process repeated until they match.

computer system detected errors of typography and nomenclature, so those trials "went up in smoke," as Kepler might say[2] (see Figure 3).

This was followed by a series of runs in which other logical flaws were detected—for example, there turned out to be an error in the Robert Small book, which I had blindly followed. By the sixth try, I already had in hand one very interesting result, after a total of eight minutes of computer time. In the ensuing runs, I corrected several more errors and also computed with different initial conditions, as I shall explain. Altogether, I used 12.4 minutes of IBM-7094 time. Now that the program has been written and "debugged," additional cases require only the eight seconds quoted above. Figure 4 illustrates an example of the output.

[2] "Itaque causae Physicae cap XLV in fumos abeunt." [4a]

The results I have just quoted sound more like a publicity release for electronic computers than a serious paper in the history of science. However, one quite remarkable fact turned up in this investigation. Instead of requiring seventy trials as Kepler did, the computer program, using identical methods, took only nine trials! In fact, we might have anticipated this result without doing any calculations at all, from the following considerations. Suppose the aphelion and the zero point of the mean longitudes are originally known to 1° (actually they were much

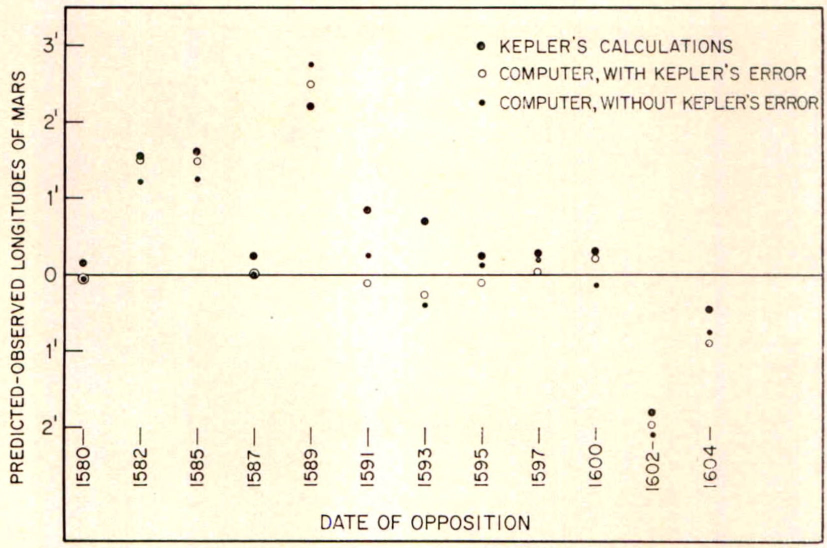

FIG. 5

better known than this). Suppose we wish to get these to 30″ of arc, that is, an improvement by a factor of 120. Since 2^7 is 128, 7 inner iterations should be required in each of 7 outer iterations, if the error is halved each time. This total number of iterations, about 50, should probably be halved because the inner and outer iterations are not independent, and as the outer iteration converges, the inner set will require fewer than 7 tries each time. Furthermore, since the problem turns out to be fairly linear, we can use proportional parts to speed the convergence, and hence we might again halve the number of iterations, making about 12. On the other hand, we make an initial try, then a try with an arbitary displacement, and finally a try with proportional parts based on the first results. Thus, three tries in each inner iteration, and three outer iterations, give a minimum of nine trials by this method, precisely the number used by the computer.

Why, then, did Kepler require seventy trials? Since Kepler already started with an arbitrary correction to Tycho's zero point on the mean

longitudes, we suspect that he may have used many trials to reach the starting point shown in *Astronomia Nova*. Therefore, the calculations were repeated, starting directly from Tycho's figures. Now, thirteen iterations are required, still a very small number.

I can only conclude that Kepler was horribly plagued by numerical errors, that his trials accidentally diverged nearly as often as they converged. No wonder he was so frustrated in his attempt to solve this problem, which was apparently just at the limit of his computational ability! Do we have any evidence for this conclusion? Yes. At the very

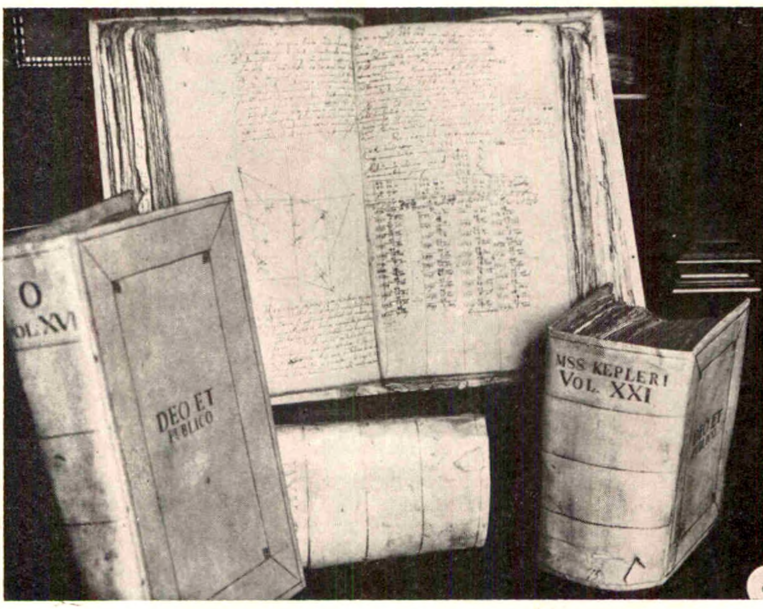

FIG. 6. Kepler's original manuscripts, including the 900 pages of Mars calculations, are still preserved in Leningrad. "Deo et Publico" is the motto of Catherine the Great, who purchased the volumes for the Russian Academy of Science in 1773. Photograph courtesy Phillips Library, Harvard College Observatory. [10]

beginning of his calculation, Kepler makes numerical errors in three of his eight starting angles—errors of the same order of magnitude as the corrections he was seeking. These errors were noted both by Small and by Delambre. I therefore programmed the computer to solve the problem both with and without this initial error. The final solution appears comparatively insensitive to these errors, but it is curious to note that Kepler gets about the same answer *with* the errors that the machine computes *without*!

After Kepler completed his solution with four of the twelve oppositions, he carefully calculated the predicted positions for all twelve ob-

servations [5]. The results, shown in Figure 5, exhibit several interesting features.

First of all, since the solution was carried out exactly for the oppositions of 1587, 1591, 1593, and 1595, the same observed positions ought to be predicted by the theory. But here, Kepler has taken a very curious step: he corrects each of the positions for the advance of nodes of Mars—a curious step because the correction is made *after* the main calculation instead of *before* [6]! Thus, only the pivotal 1587 opposition must predict exactly the observed position; yet, as the graph indicates, Kepler has made a small computational error of 15″. Given a uniform motion of the nodes, the 1591, 1593, and 1595 observations should show increasing errors, yet again this is not the case. Compared to the machine calculations, Kepler's results for 1591 and 1593 show computational errors as large as 1′. One final comment: note from the graph how Kepler's errors generally increase the deviations between observation and prediction, *except* for the most discordant cases!

The best possible solution with this type of model, as stated previously, leaves errors up to 8′ of arc. We see here that Kepler was incredibly lucky in his particular choice of observations—or perhaps we should say unlucky, because, with larger errors, he would probably have recognized the inadequacy of this construction earlier. As a test, I chose other well-distributed sets of four oppositions as the basis of the solution, and I indeed found larger errors, up to 8′ of arc.

I hope this study has shed some light on the difficulties encountered by Kepler, and perhaps on his computational ability. My thesis, that his calculations were incredibly loaded with numerical errors, has already been observed in another section of *Astronomia Nova* by O. Neugebauer [7]. Perhaps it will someday be further confirmed by a full analysis of the 900 pages of original manuscript computations, still extant in Leningrad [8]. I do not wish, however, to detract in any way from the magnitude of Kepler's scientific achievement. Perhaps the most appropriate conclusion would be a further quotation from *Astronomia Nova:*

"There will be some clever geometers such as Vieta who will think it is something great to demonstrate the inelegance of this method. (As a matter of fact, Vieta has already made this charge against Ptolemy, Copernicus, and Regiomontanus.) Well, let them go solve this scheme themselves by geometry, and they will for me be a great Apollo. For me it suffices to draw four or five conclusions from one argument (in which there are included four observations and two hypotheses), and to have shown by the light of geometry an inelegant thread for finding the way out of the labyrinth. If this method is difficult to grasp, how much more difficult it is to investigate things without any method" [9].

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AMERICAN SCIENTIST, *52*, 1964

RIDDLES OF THE TERRIBLE LIZARDS*

By GLENN L. JEPSEN

MANY unsolved mysteries of dinosaurs continue to haunt the minds of paleontologists. Libraries of fact, speculation, philosophy, and scientific conclusion have been devoted to dinosaurs but some of the most exciting and penetrating studies and deductions on their anatomy, ecology, physiology, and genealogy are yet to be made. Dinosaurs are all dead, of course, and have left no direct living descendants that can be critically examined. Hence, much of the more interesting information about them is derived through analogy and not by direct observation. No vertebrates that were alive two centuries ago are living today but most of their genetic lines of evolution are still here for study and experiment. Three hundred and fifty thousand times this mere two-hundred-year moment have elapsed since any of our ancestors saw a living (or a recently-dead) dinosaur, and we retain not a trace of "racial memory" or even of clan gossip to signify the fact that dinosaurs and pre-people knew each other, as they almost surely did. Nor do we have any other extra-sensory means of avoiding the labor and cost of the scientific sleuthing that is necessary in the study of long-gone reptiles. Information about them increases knowledge about our dead selves.

Dinosaurs never intended to become extinct, of course, and didn't even know when their racial knells were sounding although the tolling continued for several millions of years of racial decline and death. Even with our modern intelligence, if we had lived with dinosaurs in the Age of Reptiles we probably would not have known that they were doomed until they were very near the end of existence.

All literate children now learn at an early age, but are reluctant to believe, that the friendly dinosaur followed a well-trod trail to oblivion. Junior naturalists have heard and read numerous other tales, many of which are alluring but untrue, about the monsters with long jagged names, that seem to have replaced for young imaginations the exquisitely frightful ogres and dragons in the fairy stories of earlier generations. Being extinct, dinosaurs are rather benevolent and easily tamed and yet excitingly awesome.

In a recent survey sponsored by the National Science Foundation, paleontology was discovered to be the subject that grade schoolers most want to read about. The survey also found that school libraries are badly out of step with student interests. Paleontology was number three when ranked by interest of both high school and grade school readers

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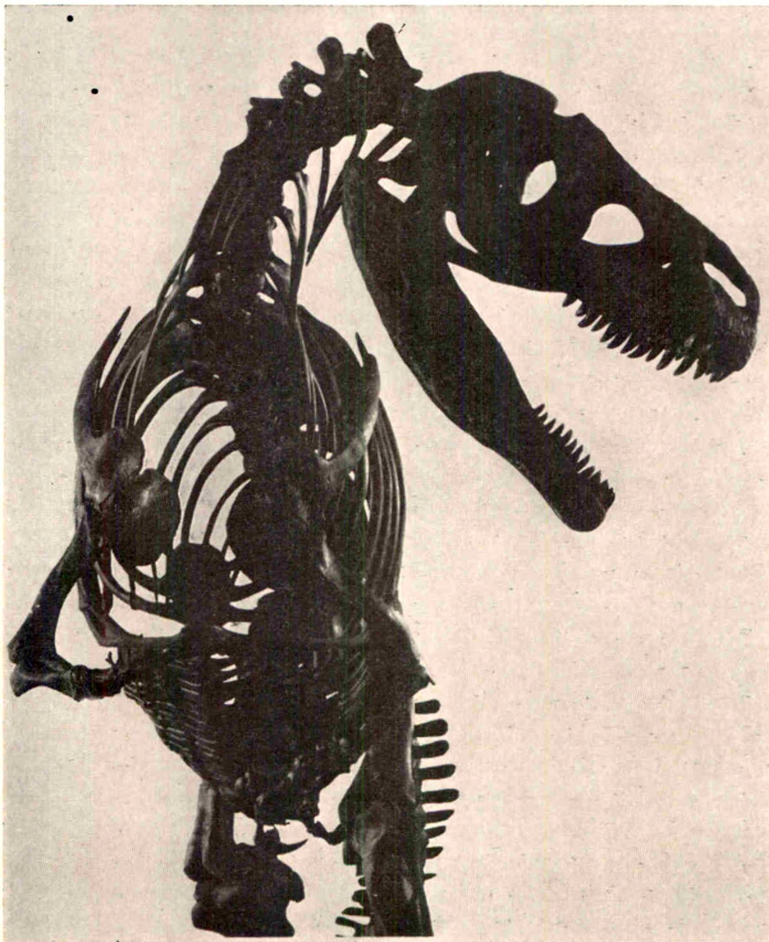
* Revised, by permission, from *Princeton Alumni Weekly*, *64*, 6, 1963.



DYNAMIC DINOSAUR. Resurrected fossil skeletons frequently evoke revealing human reactions. Variously, people in a brief opinion-survey saw in this Jurassic reptile (1) a vigorous and determined defense of territory or of self, (2) the surging muscular tensions that precede the anticipated attack of another predator, (3) the satisfaction of a flexing yawn, (4) an over-the-shoulder snarl at an irritation, (5) a hearty guffaw, (6) the loud vocal instruction "Stop, now, —that tickles'" and (7) a pleading shout of "Allons!" to followers.

but only number eighteen in rank in permanent book collections. (Botany was number four in book holdings but only number nineteen in reader interest.)

Even so, despite this great upsurge of youthful demand (by people of all ages) for knowledge about dinosaurs, far greater now than at any time since one was first described nearly a century and a half ago, many of the most interesting details about them form tantalizing problems for future research with new methods of microscopic and geochemical analysis and with more exploration for additional specimens.

A long list of significant projects for further research could be compiled to indicate how little is known about some aspects of the dinosaur's stay on earth. Here are a few.

We don't know whether most dinosaurs were ovoviviparous as rattlesnakes are (young hatched from membranes within the body and born alive), or hatched like alligators from external eggs. Some dinosaurs did lay eggs; shells have been dug up by the baker's dozen in Mongolia and France, and fragments are known from East Africa, China, Portugal, Brazil, Alberta, Montana, and Wyoming.

Pounds or Tons per Year?

What was the life expectancy of a dinosaur? How many years did a big one live? Did it reach full size in fifty years or require a thousand years or two for the accumulation of fifty tons of body? If dinosaurs grew at normal reptilian rates and ate only the faunal or floral foods that are known to have been available to them they must have lived a very long time. Nutritional requirements for an almost full grown *Diplodocus,* at average reptilian metabolic rates, suggest either that vegetables or animals of extremely high protein content were abundantly available or that the creature had to convert food to flesh for many centuries. (This, of course, is a heretical statement because everyone knows that *Diplodocus* and *Triceratops* ate plants. Or did they?)

A recent article in *Science* by an authority on dinosaurs describes evidence which leads him to think that the big sauropods were omnivorous. And Dr. T. E. White, a scientist at Dinosaur National Monument, has come to the conclusion that even the largest forms were at least as active as modern elephants which have to spend most of their time eating in order to generate requisite levels of energy.

Modern observations on dinosaur bone yield hope that the problem of growth rates can be solved, despite several technical difficulties of investigation. Layers, similar in appearance to annual rings in plants, have been found in dinosaur bones from Utah and Wyoming, and probably occur in most of the extinct saurians. Just as some tree trunks and limbs grow diametrically by the addition of woody material in the cambium zone, so do limb bones and other parts of the skeleton increase in size

by deposition of bone from the periosteum which surrounds them.

Change in the rate or kind of deposit may cause seasonal layering. In a few pieces of dinosaur bone the laminae appear in remarkable clarity as a series of pairs of alternating dark- and light-colored layers. Perhaps each pair represents an annual rhythm of food changes or of metabolic fluctuations associated with reproduction. Dinosaur bones

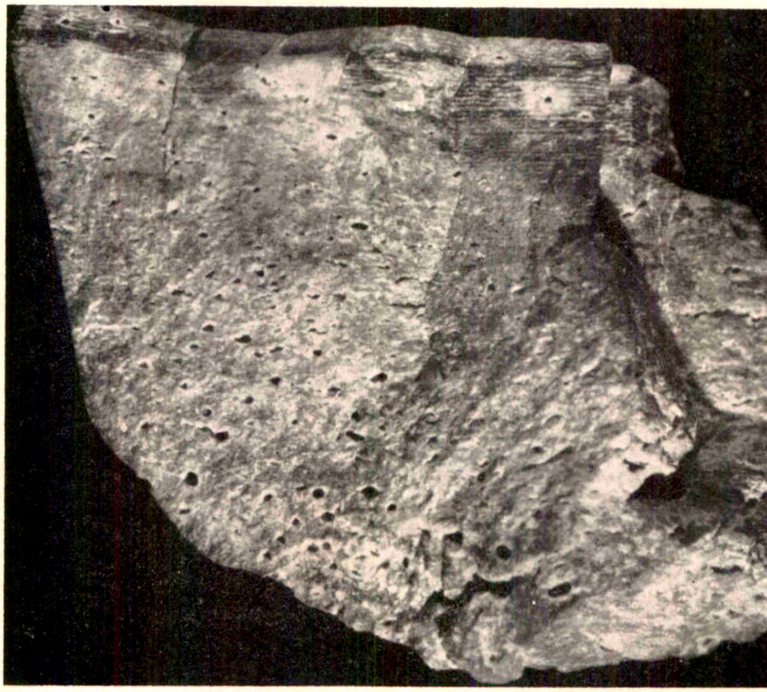


FIG. 1. Fossilized bone with about 120 pairs of layers (osteochrons), late Cretaceous (75,000,000 years old), part of 'frill' of head of ceratopsian dinosaur from "Lance" formation, Polecat Dome, N.W. Wyoming. Each pair of layers (one light and one dark) measures about 1/6 mm., or 150 pairs equals one inch. Holes are nutrient channels that contained arteries, veins, lymph vessels, nerves. (Photo enlarged *ca.* 4×.)

and other old fossils still contain amino acids and other vital juices which, if we can devise sufficiently delicate systems of microanalysis, may help define the conditions of formation of the layers. Within a few years a small new scientific field, possibly called osteochronology, may become significant in indicating events in the daily or yearly lives of dinosaurs. Small concentric scales of dead bone may reveal for a moment the pulse beat of geologic time. Is this study bringing into focus and winding a tiny delicate petrified clock within the massive old bones?

While confessing ignorance about the length of time nature required to build a dinosaur, it is pertinent to ask why certain of its structures

were formed at all. We have, for example, no remotely satisfactory explanation for the function of the ten-inch thick bone in the cranium of *Pachycephalosaurus*, and we should examine again our assumption that the winding channels in the fan-frilled heads of duck-billed dinosaurs were air traps that prevented water from entering the lungs.

Why Become Extinct?

By far the most baffling major question about dinosaurs is—What caused their extinction? Dozens of theories make an extensive literature of speculation on this intriguing subject. They range in tenor from scientifically thoughtful and analytic notions to those with mystic and facetious intent.

Authors with varying competence have suggested that dinosaurs disappeared because the climate deteriorated (became suddenly or slowly too hot or cold or dry or wet), or that the diet did (with too much food or not enough of such substances as fern oil; from poisons in water or plants or ingested minerals; by bankruptcy of calcium or other necessary elements). Other writers have put the blame on disease, parasites, wars, anatomical or metabolic disorders (slipped vertebral discs, malfunction or imbalance of hormone and endocrine systems, dwindling brain and consequent stupidity, heat sterilization, effects of being warm-blooded in the Mesozoic world), racial old age, evolutionary drift into senescent overspecialization, changes in the pressure or composition of the atmosphere, poison gases, volcanic dust, excessive oxygen from plants, meteorites, comets, gene pool drainage by little mammalian egg-eaters, overkill capacity by predators, fluctuation of gravitational constants, development of psychotic suicidal factors, entropy, cosmic radiation, shift of Earth's rotational poles, floods, continental drift, extraction of the moon from the Pacific Basin, drainage of swamp and lake environments, sunspots, God's will, mountain building, raids by little green hunters in flying saucers, lack of even standing room in Noah's Ark, and paleoweltschmerz.

Another way to speculate out of the dilemma of explaining cause is to infer that dinosaurs aren't yet actually extinct—that some may be still around, possibly evolved into new forms, living 'way off in caves in some remote and unexplored part of the world, presumably in a climate much warmer than that required by the Abominable Snowman, or by the unverified monsters that are said to lurk in the Lochs of Scotland. As remoteness dwindles, however, so does the number of people who hold this view.

In an attempt last year to spotlight the final exit of the terrible lizards, Rickey Eckler wrote his Princeton Senior Thesis on "Extinction of the Dinosaurs," hoping to see further into the subject by analyzing the factors of Mesozoic life and death. To test the distribution and variety of current

theories on the demise of dinosaurs, he wrote to many specialists in paleontology to ask "What caused the extinction of dinosaurs?" Only one strong consensus emerged from nearly three dozen thoughtful replies—"No one knows." In response to this challenge he may extend the investigation into a doctoral dissertation and perhaps a lifetime of geo-bio-chemical research. Ashes to ashes, and dust to dust, but how?

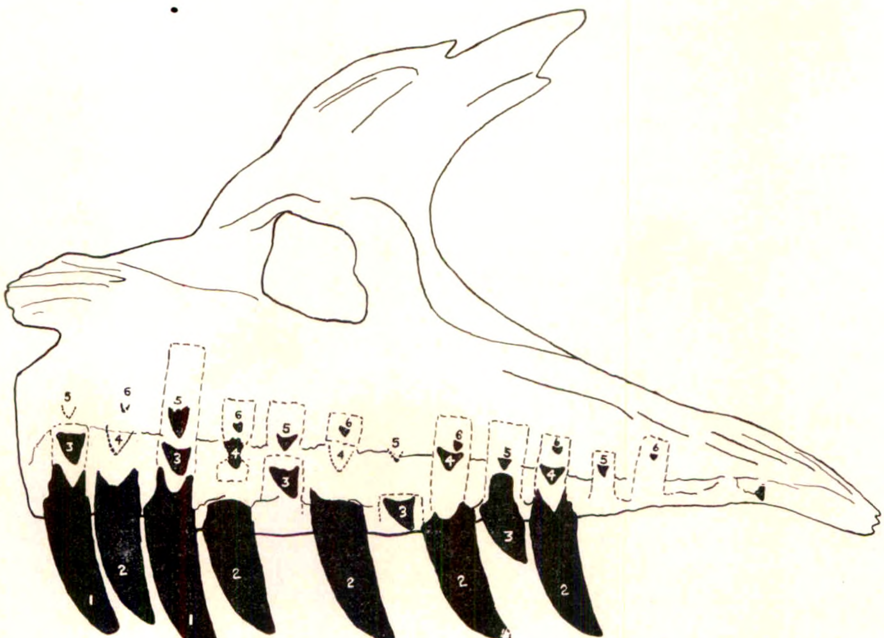


FIG. 2. Diagram of part of right face-bone or maxilla (front toward left) of *Antrodemus* as seen from inside of mouth, with the inner bone removed to expose the roots of old teeth and germs of replacement teeth. The strong parallel 3-inch-long saw-edged knives formed, with the lower teeth, a dagger-scissors system to chop gulp-sized pieces of tough dinosaur meat. Bone around each socket was precision-funnelled for exact positioning of an unlimited series of teeth. Two alternating sets of teeth, indicated by odd and even numbers, erupted in sequence, old pushed out by new, beginning at the rear (right) and progressing toward the front, thus never leaving a gap of more than one tooth-space. Perhaps such paleodental surgery will reveal the rates of tooth replacement and the number of birthdays allotted to *Antrodemus*—as more research data come straight from the dinosaur's mouth.

During the past twelve months several hundred thousand school children (with adults in tow) have visited the dinosaurs in many a museum and have frequently demanded "Where'd they all go?" No one knows. No single theory can support all the factors and facts of extinction; different causes eliminated different individuals and groups at different times. New research methods, however, are pressing fresh information from the bony pulps of the past.

Little Monsters

So much superstition and myth about dinosaurs has become part of our accepted natural history folklore that only a small part of this long-accumulated mass of misinformation can be mentioned. Not every dinosaur was huge. Many dwarfy kinds were smaller than ponies or even sheep, and, of course, in a clan group or pack of any kind of dinosaur—even the giant species—there were many more small young dinosaurs or sauridions than big old bulls.

We know nothing of age-group or social structures among dinosaurs such as the ratios of hatchlings to teenagers to the mature. Dinosaur demography is an almost unexplored field, and sauridion mortality rates are not likely to be known soon because the records are fragmentary and scattered. One vague age-ratio record has puzzled dinosaur hunters through many years of careful exploration. In the rich fossil fields of Utah, Wyoming, and Tendaguru where adult dinosaurs are abundant the rocks contain only a few bones of sauridions. A common way to explain this observation or to escape from it is to state that the youngsters were kept in upland brood areas safe from the harsher forces of nature (as well as from burial and future research in museums) while the grown-ups foraged on plants or on each other along the shores of lowland rivers and lakes. Senior citizens were thus readily buried after death, with consequent riparian rights to become fossils imbedded in stone.

This vision, for which the lullaby might be rephrased to "Rocks avoid baby on the hill top," is truly a hypothesis based upon negative evidence—the absence of remains of young dinosaurs. Some day it may be verified by positive evidence when bone diggers' picks open dinosaur nurseries strewn with the small dainty bones of sauridions. Come soon the day!

Lest the thought of these tender stony ghosts wring too much mammalian sympathy from us, it is well to remember that the family life of reptiles is thin and sharp. After mother dinosaur laid eggs she might not even remember where she did so, when a hatch occurred, or what it is that hatched, and if her young were born alive they precociously took off to make their own living—almost at once, quicker than you can say Horatio Alger Jr. And if a reptilian father was there, the local infant mortality rate probably increased instantly. Thus, the pastoral scene in paintings or movies, showing members of the *Triceratops* clan peacefully dispersed in little family groups of father, mother, and the siblings is a pictorial superstition. Dinosaurs undoubtedly had many troubles but they didn't suffer from momism. Young ones were completely organized chips off the old blocks,—even in details of dental hardware. (Fig. 2.)

"Eight-Foot Eggs?"

Superstitions about the size of dinosaur eggs are also common. "How did they lay those big eggs?" is a frequent question by students as they

examine the rear ends of large dinosaurs. It is based, of course, upon the belief that egg size was correlated with adult size. If a Mongolian *Protoceratops* (the only known dinosaur that is well-represented by eggs, juveniles, and adults) increased in bulk about two hundred times from when it was an eight-inch long egg until it became a six- to eight-foot long adult, so the reasoning goes, then a brontosaur egg and a grown-up probably had comparable size ratios. In extrapolating from small to big species with this absurd analogy it may be imagined that a 50-ton adult *Brachiosaurus* laid an egg weighing two and one half tons in a shell seven or eight feet long.

By a curious and ridiculous coincidence I once received a frantic request from Europe for me to verify a newspaper report that we had collected in Montana some whole dinosaur eggs that were eight feet long! It was a pleasure to reply that our discovery consisted of small calcareous egg-shell fragments whose curvature suggested that the whole eggs had been not more than eight *inches* in greatest dimension.

Calculations indicate that the eggs of the extinct bird *Aepyornis* of Madagascar or of the French dinosaur *Hypselosaurus* may be near the top limit of size for eggs—10 to 13 inches long. In larger ones the internal fluid pressure would be so great that the shell would either have to be held together by cross struts or of such great thickness that an embryo would require a hammer and chisel to get out at hatching time.

Most people believe that all dinosaurs were excessively stupid—that they had no brain power at all and that this was probably the major cause of their downfall. This reasoning fails, not only because many other animals that lived during dinosaur times and continue to flourish to this day possess even less gray matter, but also because we don't yet have satisfactory ways of measuring comparative effectiveness or size requirements of brains for mere existence. Like many legends about animals this one has the common anthropic overtone that permits man (whose brain size is thought to be far greater than it needs to be for minimal survival in nature) to emphasize his superior intelligence. If dinosaurs were witless, they had been so for 120 million years before the scythe of time finally mowed them down, and the question arises—Why did this old long-continued dumbness finally cause their doom? Brains are like purses; contents are more important than size.

All kids know that in the central nervous system of the pelvic region in some dinosaurs (with upright sheet-bone plates along their backs and heavy long sharp spikes on their tails) there was an enlargement which exceeded the size of the animal's brain by twenty times. This kind of plexus, found in many dinosaurs in both the hip and the shoulder regions, probably made a quick motor response possible for the limbs and tail. Without these substations in the neural communications controls perhaps as much time as two seconds would be required for a nerve impulse

to travel from the tip of a brontosaur's long tail which was being nipped by a hungry predator (on a day when the temperature was 70 degrees) to the brain and back with orders for action. A lot could happen in a whole thirtieth of a minute—our subject might be painfully detailed.

Comic Strip Anachronisms

To the discomfiture of vertebrate paleontologists who have to answer endless questions about dinosaurs, the belief that "those big reptiles" and man lived at the same time on this planet is widely held, thanks in

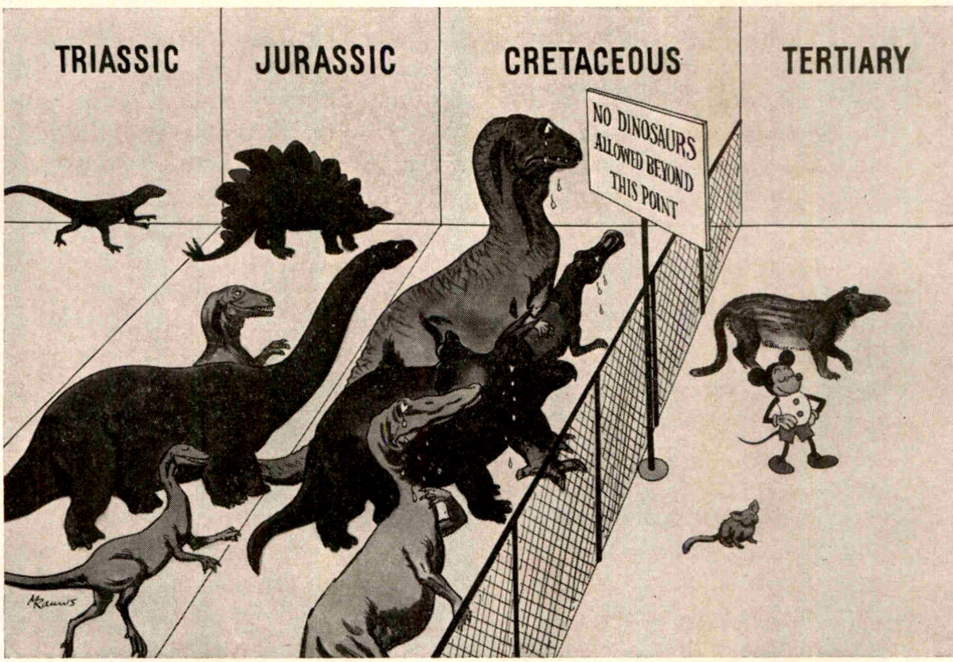


FIG. 3. Artist Michael Ramus' epitome of dinosaur history through Mesozoic time, satirizing current superstitions about anthropic emotions and the extinction of the giant reptiles.

part to the comics, pseudoscience fiction, movies, puppets, toys, kits for the construction of prehistoric scenes in plastic, and television. Alley Oop, B. C., The Flintstones, and "The Monster in Manhattan" all have a share in creating and sustaining the idea.

Dinosaurs are natural subjects for many lighthearted allegorical distortions in children's books, records, funnies, and films. Few (perhaps no) other animals have served more faithfully to keep children, who know much more about dinosaurs than mother does, "out of mother's hair."

These same youngsters recognize this saying as an idiomatic cliché which brands mother as a mammal. As junior is apt to explain to her, literate dinosaurs would never make such a remark because dinosaurs, being reptiles, don't have any hair to keep anything out of. To the average grown-up who can take dinosaurs or leave them alone, they have become rather amiable clodpoll buffoons, not to be exactly ridiculed or censured but not to be taken very seriously, either; amusing, every ton of them. In a western "Dinosaur Park" a herd of assorted life-size models of dinosaurs are locally called, with affectionate and punful disdain, "those darn eyesores."

Many people who know better than to believe in the 70-million year anachronism of people-dinosaur contemporaniety (either that people lived in Mesozoic time, or dinosaurs are still alive somewhere), do, however, think that all dinosaurs died in a hurry, within a period of a few, or a few thousand, years. Geologists themselves must take much of the responsibility for the dissemination of this concept because they have often defined the end of the Age of Reptiles or Mesozoic Era as the exact time that dinosaurs became extinct. Ergo, reasoning in a tight circle, dinosaurs became extinct at the end of Mesozoic time.

"Flapping Wings" and "Stomach Stones"

Paleontologists in classrooms and museums often carefully emphasize the facts that there were no races or species or genera or orders of dinosaurs that ever learned to fly and none that dwelt in or on marine (sea and ocean) water, although some did spend at least part of their time in lakes and rivers. Numerous essays of students everywhere however, continue to repeat lurid stories of "dinosaur denizens of the deep" and "huge flapping wings of flying dinosaurs." Some years ago, one of my students dutifully tried to put in a plug against this common impression by stating in a test, "dinosaurs had no flying orders." True.

One myth about the eating and digesting habits of dinosaurs has been profitable in hundreds of western souvenir shops where shiny "gastroliths" or "stomach stones" are sold, usually for a quarter apiece. If there were an unlimited market for these at a tenth as much, we'd easily have all the funds necessary to build a new and superior geological museum on the campus of every college, because some thick and extensive gravel beds in Utah contain "gastroliths" or highly polished rounded pebbles by the billions—with not a dinosaur in sight.

Pebbles that have been found inside the collapsed rib cages of dinosaurs or in undisputed association with other extinct reptiles (such as plesiosaurs) never have a bright lustrous surface but are invariably dull or frosted in appearance, as are the small rocks and broken pieces of coke bottles and other bits of debris that are found in zoos at autopsies of deceased crocodiles and alligators. Fine gravel in the crops of fowl

becomes similarly coated with minute traceries and scratches as they grind together in the digestive mill. The origin of the sheen on the pseudogastroliths has long puzzled geologists, although siliceous pebbles on the surface of the ground in some places today occasionally attain a similar glossy texture when they are gently abraded for a long time by particles of clay and silt carried in a flow of air or water. Dr. Sheldon Judson is now conducting a detailed study of surface features on pebbles to determine modes of formation of the kinds of gloss that were produced within dinosaurs and elsewhere.

So generally held and so alluring is the belief that these rocks glitter by virtue of their sojourn inside of dinosaurs that it will be a long time before "gastroliths" are properly debunked to become "gastromyths" in the popular lore of dinosaurs.

The "Romantic" Paleontologist

When thinking of superstitions another one comes to mind—the almost universal belief that the search for dinosaurs and the task of exhuming them from their burial vaults of investing stone is an exciting, highly romantic, and easy occupation. Exciting it sometimes is, but the infrequent "romantic" moments occur briefly between long periods of hard work and discomfort.

Diggers, working from soon-after-dawn to dusk in rocky deserts, are usually coated with sweat-smeared earth dust, much closer to Stoic than to Roman tradition. Well worth the effort and the muscle-hurting fatigue, however, is the sensation of discovery, the burst of exhilaration at uncovering a new kind of fossil bone or tooth, a chip of the cosmos that has been sealed away in a natural treasure-vault for a hundred million cycles around the sun.

This rare moment of flighty romance is likely to be interrupted by lunch, a dry solar-heated sandwich softened enough by gulps of warm water to swallow. Vertebrate paleontologists see red when someone exclaims, "What a lovely vacation it must be to hunt fossils! And a picnic lunch out in the open air every day!" A truer midday picture shows the weary excavators sprawled under the truck in the precious small shallow pool of shade it makes from the white fire disc overhead, as they try to recoup enough energy for a burning afternoon with pick and shovel.

Satisfactions of Science

Corollary to the myth of the simple easy holiday in the search for fossils is the fact that bone digging actually does have some rewards which are not generally mentioned or discussed by bone diggers for fear of revealing a ready sensitivity to beauty in nature. In our current cult of preoccupation with numbers, quantities, and computers and other gadgets of academic automation, it is somehow out of pace with the times

to admit to aesthetic pleasures in Science. Nevertheless, as the successive layers of matrix are cleaved away to expose a petrified bone of a carnivorous dinosaur, a paleontologist may thrill to see it as a beautifully formed organization of elements that have served many functions in earlier eons. Milliards of milliards of years ago the particles of substance in this gracefully contoured structure were not yet even part of a galaxy—only of diffuse matter in an endless firmament. As recently as ten billion little years ago the elemental atoms could not have had the framework of what we know as life, but recently, only a hundred million years before our moment, some of the stuffs now in this limb bone had helped build a graceful green living plant. They were next consumed to become part of the intricate body architecture of a dinosaur vegetarian, only to be devoured and redesigned as a carnivore. Muscles, nerves, vibrant motion, and the intertwining strands in life's cable of continuity are all implied by this bare bone as it rises in awed hands from its planetary tomb, resurrected to help teach minds and engender new thoughts. This elegantly structured thing will briefly stay in this shape in a museum where, hopefully, muses will also come, and, perhaps, an embryonic Darwin. Later, fractions of such a temporary form as a bone will be dissociated and will flux with other shreds of the universe, constantly interforming through limitless time, from one dust to another, from unpredictable future flowing into continuous past.

Diggers of fossil bones, caught off guard, will firmly answer "Yes, endlessly, forever," to the question "Son of man, can these bones live?" Death doesn't last very long in the thin veneer of the earth's surface; materials of life are in steady demand for composing the rhythmic cycles and kaleidoscopic fluctuations of evolutionary products.

Students in search of probing questions often ask if an allosaur will ever appear again, if the necessary chemical materials can be assembled and organized by nature in the future to recreate this specific living reptilian predator. The answer is no. Evolution cannot backtrack on such a grand scale, nor can it even appear to do so. There is far less possibility that the complexly interreacting galaxy of genes which participated in the genesis of *Antrodemus* will evolve again than that someone will some day find and reassemble to original form the dust and chips that Cellini removed from a block of lapis lazuli to form a sparkling blue bowl. The message about conservation of the records of living and of long-gone animals and plants is obvious.

Reclaim the Only Past

Present federal and state laws do not adequately protect our national heritage of precious petrified dinosaur bones from destruction in western states by amateur fossil hunters who, with no knowledge of the past and no thought of the future, recklessly smash exposed bones and unearth

others from their natural graves to gloat ignorantly over these "trophies." Confusion in the minds of many people, legislators included, about the difference between archeology and paleontology (which need separate and special regulations) has hindered the development of scientific security regulations that might defend dinosaurs from the fate, off Bering Island, of the giant (thirty-foot long, four ton) Steller's sea cow, which was unable to dive away from the harpoons of predatory "sport"-loving

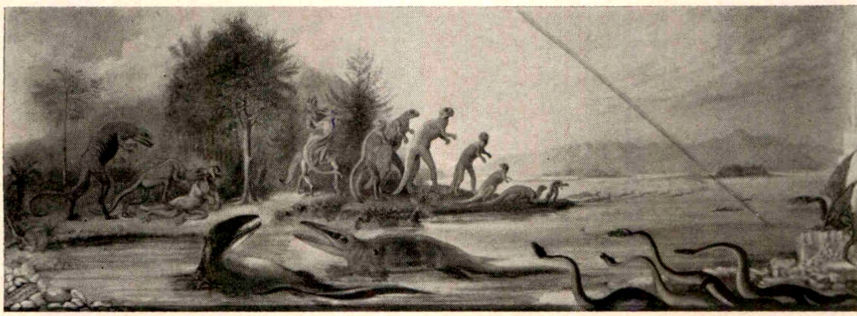


FIG. 4. New Jersey landscape in the Cretaceous Period as depicted by B. Waterhouse Hawkins in 1877. This was one of the earliest attempts to reconstruct and place fossil vertebrates in life scenes. Many details in this picture were later proved to be wrong but it established a pattern of ideas in prehistoric pictures that became traditional. In the foreground a foursome of snaky plesiosaurs watches a pair of ichthyosaurs (none of which should be out of the water) conversing in mild shock about conduct in the long line of dinosaurs, one of which is expiring in operatic fashion with a villain at its throat while another pair presumably practices a vigorous and advanced version of the paleotwist. The pterodactyl narrator-prompter at the right appears anxious to announce further events.

sailors and, victim of greed, has completely disappeared from the face of the earth. The number of earth-bound dinosaurs is likewise limited, and their exhumation should be directed henceforth only by experts who have access to the knowledge and the laboratories of trained geologists, biologists and chemists. It is now time to stop the plundering of our irreplaceable ancient treasures that are sepultured in stone.

Wilderness-roving professional fossil hunters, clad in sweaty clothes and steeped in long-range visions of time and substance, often reflect upon (but almost never express) the kind of comment that Arnold Guyot made in Princeton more than a century ago, "The spectacle of the good and the beautiful in nature . . . calms and refreshes the soul."

Some people cherish the fancy that trained fossil-bone collectors are curious and obscure kinds of morticians or grave diggers in a macabre occupation. Paleontologists themselves, however, are much more concerned with the fact that ancient vertebrates once lived than that they died; the effort in the discipline of studying fossil vertebrates is to resurrect and revive the living part of the past, not to bury it.

Two closely-related and persistent folktales hold the belief that each dinosaur skeleton is (1) 99% plaster of Paris and (2) 99% imagination—that the only real part is perhaps a small toe bone from which a wild-eyed paleontologist with baggy pants invented the whole mad framework of bone, and that he ran out of plaster or the specimen would be bigger. But here the bone digger is on firm and well-proved ground because the skeletal designs of many dinosaurs are thoroughly known. Indeed, we know more about the form and fit of the stony bones of certain dinosaurs than we do about the supporting structures of some living animals.

When dinosaur remains were first studied and assembled, and the demand for examples far exceeded the supply, many "plasterotheres" were cast and marketed. A few of these are still preserved, more for the sake of history than for information. One of them perished in Princeton in 1909 when a plasterothere-hating curator was installing the fossil collections in the newly built Guyot Hall. He devised a series of ingenious accidents that shattered the artificial bones of gypsum beyond redemption. This skeleton had formerly resided in the present Faculty Room of Nassau Hall where it was posed in a frozen awkward lunge toward a life-size replica of Mercury who, caduceus in hand and sandal-wings extended, was quite understandably sprinting for the door.

The skeleton, a duplicate in plaster of the first nearly complete dinosaur found in North America (near Haddonfield, N. J., in 1858), was presumably wired together by Mr. B. Waterhouse Hawkins who had previously made a life-sized reconstruction of a living dinosaur in London and, as a publicity stunt, held a dinner party inside of it. His pictures of ancient landscapes were so popular in London's Crystal Palace that he was commissioned to make a second (and now, only surviving) set for Princeton. He also started a series of full-scale restorations of dinosaurs for a large building in Central Park in New York. This was never finished, and the incomplete dinosaurs were presumably buried in Central Park where future archeologists may be in for a curious surprise.

Much in a Name

Additional cherished bits of misinformation cluster around dinosaurs' names, many of which are now almost common household words, but are regarded, especially by awed parents, as unnecessarily long jaw-breakers with unpleasant sounds, whimsically bestowed by academicians who are as dry as the bones of the species being named. Many of the names are indeed ugly, and hard to say—*Szechuanosaurus* and *Psittacosaurus*, for bad example, although they are not less milli-fluous than some of the names in almost any football program. Other names of dinosaurs can be pronounced, in accord with Hamlet's instruc-

tions to the players, trippingly on the tongue; such are *Tornieria, Coelurosaurus*, and *Manospondylus*.

Dinosaur names are among the best of pawns for playing one-upmanship in grammar schools and junior museums. Any child can intimidate almost any parent by remarking that *Saurolophus osborni* is a species of dinosaur from Alberta that was named in honor of Henry Fairfield Osborn.

"Saur" is rooted in the Greek word for "lizard" and "dino" means "terrible," a fact apparently overlooked when people are urged to buy "dinofuel" for their automobiles and thus subscribe to the common belief or superstition that all dinosaurs were swift and powerful. Further, "fuel," from Middle English sources, indicates a means of sustaining extreme fondness or passion for anything. Hence "dinosaurfuel" could mean, if you don't insist upon etymologic purity, "great enthusiasm for the terrible reptiles," and an increasing number of people confess to having this passion in natural history.

For many years Princeton had no assembled dinosaur skeleton, other than the "accidentally"-destroyed plasterothere that had been in Nassau Hall (*Hadrosaurus foulki*), probably because dinosaurs are so expensive. Big ones may cost more than $100,000 from the time of discovery to the moment of ceremonial unveiling in a museum. Thereafter the upkeep is cheap and educational. The high cost of dying didn't faze dinosaurs but the high cost of their resurrection has hindered studies of them.

A Wild-Goose Chase

The presence now of a highly sophisticated mount of a splendid dinosaur skeleton in Guyot Hall resulted from an unusual and curious series of events. In 1939 Malcolm Lloyd, Jr., '94, asked why we had no dinosaur skeleton in our Museum of Natural History and offered to help support an expedition to get one. He had been told, by a person who wanted a job on such an enterprise, that a good skeleton was waiting for the diggers' shovels in southern Colorado. A summer field party was organized in 1940 to investigate. To our outraged innocence we soon discovered that the report was a barefaced fraud and that instead of hunting a dinosaur we were chasing a wild goose on a deliberately faked trail.

Fortunately an alternative real road to Jurassic dinosaurs was open in accord with our stand-by plan, and we quickly joined graduate student W. Lee Stokes (now Chairman of the Department of Geology, University of Utah) where he was already investigating a site in the middle of Utah near his home town of Cleveland in the heart of the Mormon country, at a spot from which he had previously secured bones from the Morrison formation for Brigham Young University. During that summer and the next, our digging developed an irregular quarry pattern equivalent in area to about ten yards square in the yard-thick

calcareous clay matrix. Many more than 1000 bones, representing the partly dismembered and scattered carcasses of about two score dinosaurs of half a dozen kinds were uncovered, carefully plotted on the quarry maps, and removed. Two thirds of the skeletal parts belonged to several age groups of carnivorous dinosaurs—a double reversal of the usual

FIG. 5. Bones and teeth of *Antrodemus* (an-tro-*dee*-mus) *valens* (*vay*-lens), meaning "hollow-build, strong," in the Cleveland-Lloyd Quarry, central Utah. Photo by University of Utah.

occurrence in other dinosaur quarries, of about three herbivores to one or two wholly adult flesh eaters. These facts and other testimony by the rocks and bones suggest that the animals congregated in a shallow dwindling lake or swamp area during a period of drought, that the huge starving plant eaters trampled the corpses of the dead and were them-

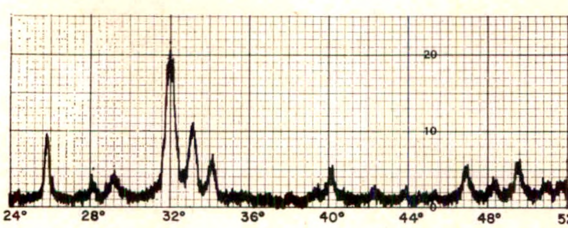


FIG. 6. X-ray diffraction trace shows that a piece of bone from *Antrodemus*' tail is now composed of carbonated calcium phosphate.

selves attacked by the desperate carnivores. A shroud of air-borne volcanic ash was later draped over the mass grave.

To designate the spot in scientific publications it was later named "Cleveland-Lloyd Quarry" in honor of the nearby town and of the man whose amateur interest stimulated professional activity.

Many of the bones in the quarry represented the predatory *Antrodemus* (also often called *Allosaurus*), to the delight of the quarriers. For

the labor of one digger, we paid dinosaur bones to his alma mater, Brigham Young University.

Extensively cracked by their postmortal residence within the earth, most of the brittle bones broke into many pieces as they were exposed in the rock and they required applications of penetrating cements before they could be removed. Even after using great care to keep all adjacent fragments together and to plot them adequately on the quarry diagrams, some of the subsequent repairs resembled the assembly of intricate 3-dimensional jigsaw puzzles. Within the cavities of some bones are clusters of crystals, translucent to milky-white, of calcite and quartz and, occasionally, radiant lavender amethyst—where once billions of rich red cells of blood were born in the marrow to pulse, laden with oxygen, through miles of great hemal channels within vast masses of muscle and bone.

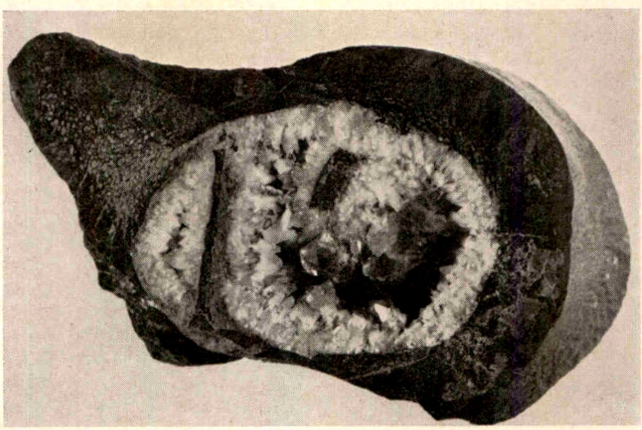


FIG. 7. Jurassic jewels—crystals of calcite, quartz, and amethyst in broken limb bone of *Antrodemus* from Cleveland-Lloyd Quarry, central Utah. Bone about 3½ inches across. At 109°F. small bubbles in the fluids within the crystals disappear, suggesting that the crystals were formed at that temperature while the bones were buried, perhaps warmed by blankets of hot volcanic ash. Photo by Willard Starks.

Were We There?

During periods of rest from the excavating work, speculations occurred to the diggers concerning the scientific and philosophical significance of the scattered jet-black pieces of skeletons. Flights of fancy led to wonder about the possiblity that members of our own squirrel-sized ancestry might have perched and chattered on top of some of these man-long bones, bleaching white in the sun, before they were buried. Our Jurassic forebears lived somewhere; for all we know to the contrary, maybe a few

of our early relatives visited this natural ossuary in the quest of food so voluminously available.

Brontosaur meat might have been delicious, either fresh or gamy, and the mountains of soft collapsing protoplasm of a single dinosaur hulk on the bank of a stream would have fed many creatures—worms, insects, fish, crocodiles, turtles, lizards, mammals, and flying scavengers, for a long time. Keen olfaction was not necessary to sense, a league or more downwind or downstream, that the flesh of another giant was being redistributed. More than 150 of the furry mammalian mites would have been required to balance one of the large dinosaur thigh bones on a scale, but in retrospect we know that the genetic line of the tiny warm-blooded forms has, here in the era of man, already endured twice as long (140 million years) as that of any of the dinosaurs in the quarry.

Many perspectives in other dimensions were also discussed in coffee-breaks around the bone pit. Beds of sediment had continued to accumulate at irregular rates in the Colorado Plateau area until at least a heavy mile-thick mass covered the bones. Then the processes reversed and erosion removed the rocks again to form this beautiful wild country, a place of interesting and turbulent episodes and memories of natural and human events. The Cleveland-Lloyd dinosaur quarry, 5800 feet above sea level lies about 110 miles southwest of the area that President Woodrow Wilson established as Dinosaur National Monument.

Names on the land within 25 miles of the quarry reflect the industrial, religious, and pioneering flavor of the country—Geneva Coal Mines, Assembly Hall Peak, Book Cliffs, Panther Canyon, Wild Cattle Ridge, and Neversweat Wash.

Putatively reliable records of the year 1897 place the origin of horse opera's most indispensable line in the mouth of a helpful pistol-toting witness when a train station was being held up for the payroll at nearby Castlegate by Butch Cassidy (who was merely following a technique perfected locally by the Robbers' Roost Gang)—"Get back in there or I'll fill your belly full of lead!"

Scattered Bones to Sophisticated Skeleton

After the precious but very heavy dinosaur bones from the quarry were freighted (without holdup) to Princeton, and cleared in the laboratory of most of the Utah matrix, more than fifteen years elapsed before several requisite factors could be funneled into focus on the plan to install a composite skeleton of *Antrodemus* as the centerpiece in our museum. Needed were funds for allocation to this vision, the development of new lightweight and strong plastics, and highly skillful experienced technicians who could make the necessary studies for modeling and casting the few missing elements of the skeleton and who had the time to work at some other museum on our specimen on a moonlighting basis—we

lacked manpower, space, and facilities. Finally these factors were favorably established and combined (probably, and regrettably, for the only possible time), and the mounted skeleton was unveiled in Guyot Hall in 1961 after being assembled there by Arnold D. Lewis and James A. Jensen who had previously taken the bones to their lab at Harvard for restorations and fittings. No other dinosaur has had a similar or more constructive sojourn in the Harvard Yard.

The jaws and the lacy skull were so skillfully restored as to create a demanding market for many copies; from the 18 institutions to which they were sent we have received a rich harvest of other specimens in exchange. Nineteen duplicates have been distributed—to museums in the United States, Canada, England, Germany, Japan, and to one student who convinced his parents that a fine lively specimen of dinosaur skull and jaws in a pose of menacing attack is far better than a mothy dead elk head over the mantel.

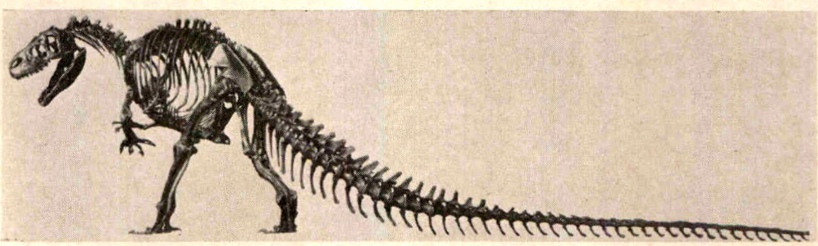


FIG. 8. Skeleton of *Antrodemus valens* Leidy in Princeton University Museum of Natural History (no. 14554), a composite of bones selected from at least three dismembered skeletons of middle-sized adults. More than 425 bones in the skeleton, which is 30 feet 9 inches long (2⅓ feet of tail—40 vertebrae, cut off by edge of picture), 22½ hands (7½ feet) high at the neck. Live weight was about 2½ tons (nearly as much as the players, referees, and head coaches at a college football game). Photo by Willard Starks.

To avoid the necessity of extending a metal rod from the platform base to the head of the mounted skeleton, it was necessary to cast the skull and jaws in tough foam plastic and thus reduce the weight from the 33 pounds of a plaster replica to 6¼ pounds. The original fossil bone would have been far too heavy for the thin supports to hold. For a dinosaur to have a cantilevered rampant pose it must be light-headed.

Several museum recipients of our head casts will also get enough bones from the Utah site, where the quarry is being extended by the University of Utah under the direction of Curator James H. Madsen, Jr., to assemble several more skeletons of *Antrodemus*. A few of these composite bodies, of somewhat different sizes, will all have heads of one size—cast from our specimen. This at last reverses the ancient practice whereby many of the Roman portrait statues (often made by Greeks) had different heads on duplicate bodies.

An appropriate final touch was given to the mount of *Antrodemus* by Dr. Donald Baird, Associate Curator and Princeton specialist on fossil footprints, when he formed several depressions in the plaster of the base, before it had hardened, to represent tracks that might have been made by the animal as it futilely trotted westward toward its original home. Near that spot several years ago a local prospector found some dinosaur tracks that he offered for sale. When pressed for more information about them he sent a photo of a large one on a thick slab of rock which he had dug up and weighed. "Think of the size of the great dinosaur that made this wonderful track," he wrote, and added, "Think of how big he must have been in life when one of his footprints weighs 562 pounds!"

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THE RETINEX*

By EDWIN H. LAND

Many visual phenomena, which have been observed over the years, cannot be explained by simple Newtonian theory. We have found that among these unexplained phenomena are the sensations perceived from experimental scenes designed to reproduce the randomness of patterns and objects in the real world. Yet, large numbers of observers have consistently reported constant visual experiences when witnessing such experiments. Accordingly, we have been searching during the past few years for some simple approach that would account for color in these situations.

We began by accepting the kind of ideas which were initiated by Thomas Young and carried on through the work of Helmholtz and Hering to such present day studies as those of DeValois, Hubel, Wiesel, Rushton, and MacNichols. All of these experimenters concluded that the retina contains receptors having peak sensitivities in different portions of the visible spectrum. In general, they hypothesized that there were peaks in the regions of approximately 600, 550, and 470 mμ and that the response curves for these receptors overlapped each other widely. Some of the later experimenters proposed that there were also what are called "opponent-type" receptors, namely, sensitive end-organs connected to cells which simultaneously give an affirmative response to the presence of energy at certain wavelengths and a denial response to its absence at other wave lengths. There is so much reason and logic in these attitudes toward the visual system, that it seems certainly desirable to accept them.

We have found it fruitful, however, to go somewhat beyond these ideas and would like at this time to suggest that these receptor systems exist in sets. We would propose that all of the receptors with maximum sensitivity to the long waves in the spectrum, for example, operate as a unit to form a complete record of long-wave length stimuli from objects being observed. (For convenience of reference let us call this suggested retinal-cerebral system a "retinex.") For us, having made this one assumption, many of the problems of color vision were enormously simplified. It then became possible to predict color in a way which formerly had been impossible. Even more important, we wish to show that, on this new basis, an entirely different rationale comes into the understanding of color vision. Curiously, color vision itself becomes such a "simple" phenomenon that the mystery and wonder of it is shifted to another domain: the domain of the perception of lightness. Much of

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* The 1963 RESA William Procter Prize Address, Cleveland, December 30, 1963.

what we will discuss in this paper, then, will be the subject of lightness, after which we will show how simply the concept of color vision is arrived at through the hypothesis that these receptor systems exist separately.

If we do make the assumption that there are several sets of receptors, for example, three, and that within each set the members are systematically related by a retinal-cerebral interconnection, so that they operate as a unit, then we can give an example of how the eye differentiates between the broad, flat absorption curves of the pigments in the real world. If a succession of band pass filters which respectively isolate the long, middle, and short wave sections of the spectrum are held to the eye, the scene viewed is perceived to differ in terms of the lightness of the objects and of the areas within it. For uniform illumination, the lightness change of the objects in the scene can be equated with ordinary spectral reflectance. Our theory would propose that the receptor sets act independently to isolate each of the different images—or, as we shall call them, lightness scales—which exist within the mixed pigments of the natural world. Although the images viewed through filters such as we mentioned would to a degree exemplify the type of lightness scale formed by the retinex, it is *only* under uniform lighting conditions, as we shall discuss, that the lightness scale on the retinex has any correspondence with measured reflected light.

Clearly, the fact that we can see these separate images with their different lightness scales by successively holding various filters to the eye does not in itself show that these images exist independently of each other when one views the world without filters. Nevertheless, we do think that, in a mathematically functional sense, these images exist independently. We are not urging that the retinal elements with the same peak sensitivity have to be *directly* connected to each other, but rather that, in the retinal-cerebral liaison, those elements with the same peak sensitivity cooperate to form a mechanism that has the capacity to establish an image. (Incidentally, this theory does not imply that "opponent-type" receptors are not utilized; to the contrary, the presence within our proposed sets of receptors of denial responses as well as of affirmative responses would only serve to strengthen the suggested reaction of these systems.)

When we look at an ordinary black and white photograph, we are seeing the response of the retinexes to a scene in which the pigments have not been separated; that is, each retinex is receiving the same image as the others in terms of lightness. When, instead of looking at a black and white picture, we look at the world of color around us, the image produced by each of the proposed retinexes differs in terms of lightness from the image on each of the other retinexes, as we exemplified when we looked at the world through the succession of narrow band filters. Each retinex system will form an image in terms of light-

ness corresponding geometrically to the optical (i.e., physical) image on the retina. Thus, there will come into being the analogue of superposition of the optical images: somewhere in the retinal-cerebral system there will exist three images in terms of lightness which have a relationship that corresponds to having three optical images on top of each other. These retinal-cerebral images in terms of lightness—these retinex images—have quite different properties from the optical image, as we suggested before. For example, light and dark areas in the lightness scale of the retinex need not derive from high and low luminous energy in the optical scale. (Moreover, in current experiments which will not be discussed in this paper, we have found that it does not at all matter how the lightness scales for functional interplay of the retinexes come into being.) Our view is that a given object in the image will coincide with itself somewhere in the multiple retinex system. This object will hold one position in the lightness scale on one of the retinexes, another position in the lightness scale on the other retinex, and a third position in the lightness scale of the third retinex. These three positions are established quite independently: what happens on one retinex has nothing to do with what happens on another retinex (in the extreme pedagogical statement of the issue.) The position in lightness, or what we will refer to as the "rank order," [1] which the given object occupies on a particular retinex is determined by the rank order of all the other objects on that retinex. And all of the positions of rank order for that retinex are determined mathematically by the interaction of the spectral absorption curve of the receptors for that retinex with the spectral absorption curves for all the objects in the field of view. Thus, the very meaning of lightness for our given object is the interplay of all the stimuli and all the sensations for that retinex alone. For our given object there exists in super-position in the retinal-cerebral tract three independently drawn conclusions—conclusions about rank order of lightness on separate retinexes. Essentially, our hypothesis is first, that the retinexes exist; second, that they draw their conclusions independently; and third, that it is the comparison of these three separate conclusions for the single object that gives the sense of color. *Color is the correlation number for several rank orders of lightness.* If we assign a number, for example, to each lightness position in each image on each of the retinexes, then the color of an object can be stated in terms of three digits, each of which expresses the lightness of a given object on its respective retinex. (As we shall see, in binary projection, two of the three digits equal each other.) At first, this hypothesis does not seem remarkably different from that which would be classically expected from the elementary Young-Helmholtz approaches to color. It is only when one examines the extraordinary stability of the lightness scale for each of the retinexes, that one comes to realize how this idea diverges entirely from the

classical point-by-point study of color vision. In classical studies, one is concerned with the mixing of energies at various wave lengths. In this approach, one is concerned not with mixing energies, but with correlating lightnesses—whatever their associated energies happen to be.

Experiments for this hypothesis are easily carried out by studying patterns or objects, in a dark room, illuminated with projectors. First, we made a large display, resembling a painting by Mondrian, by arranging a variety of gray papers cut to many different size rectangles (Fig. 1). The observer's sensation ranges from white through the grays to black in various parts of the display. This display is then illuminated by light from a lantern slide projector. By inserting in the slide carrier of the projector a neutral density wedge, we can control the illumination on our "Mondrian," and, in particular we can pick a wedge that from one edge to the other varies in transmission as much as the reflectance of the black paper in one part of the Mondrian differs from the reflectance of the white paper in another part of the array. When we insert the wedge in the projector, we see very little change. All of the various white, gray, and black rectangles look nearly as they did before. It is only by using a photometer that we can show that the energy coming from the black area at this position in the Mondrian is equal to the energy coming from the white area at this other position in the Mondrian. Although this result is well known to those who are familiar with the phenomenology of vision, it is nevertheless an astonishing event, and a good example of the difference between the lightness scale and the optical scale. [2]

A variant of this experiment is to place in the illuminating projector, instead of the wedge, what we might call a mottle slide; that is, a slide of blotches in which the transmission varies gradually in a random sort of way from place to place. When the display is illuminated with this mottled illumination, each of the rectangles making up the display looks nearly as light or dark as it did when the illumination was uniform. The observer may often not be able to tell that the illumination is indeed intricately variegated, or he may have only the sensation of a broken shadow over the board which neither affects nor interacts strongly with the perceived lightness of the rectangles. One might rationalize this magnificent accomplishment of the eye by suggesting that each area is related to an area around it and, in that way, has its lightness determined. (We must emphasize, however, that this display was arranged in such a way that no area had a predictable surround.) It seems to us, at any rate, that this rationalization would be an oversimplification of the phenomenon and that it is better to keep wondering about it than to half-explain and then to dismiss it. Our own view is that the ability here manifested by the eye to discern and to evaluate an array of simple entities in a field of violently fluctuating stimuli is one of the great basic competences of the retinal-cerebral system.

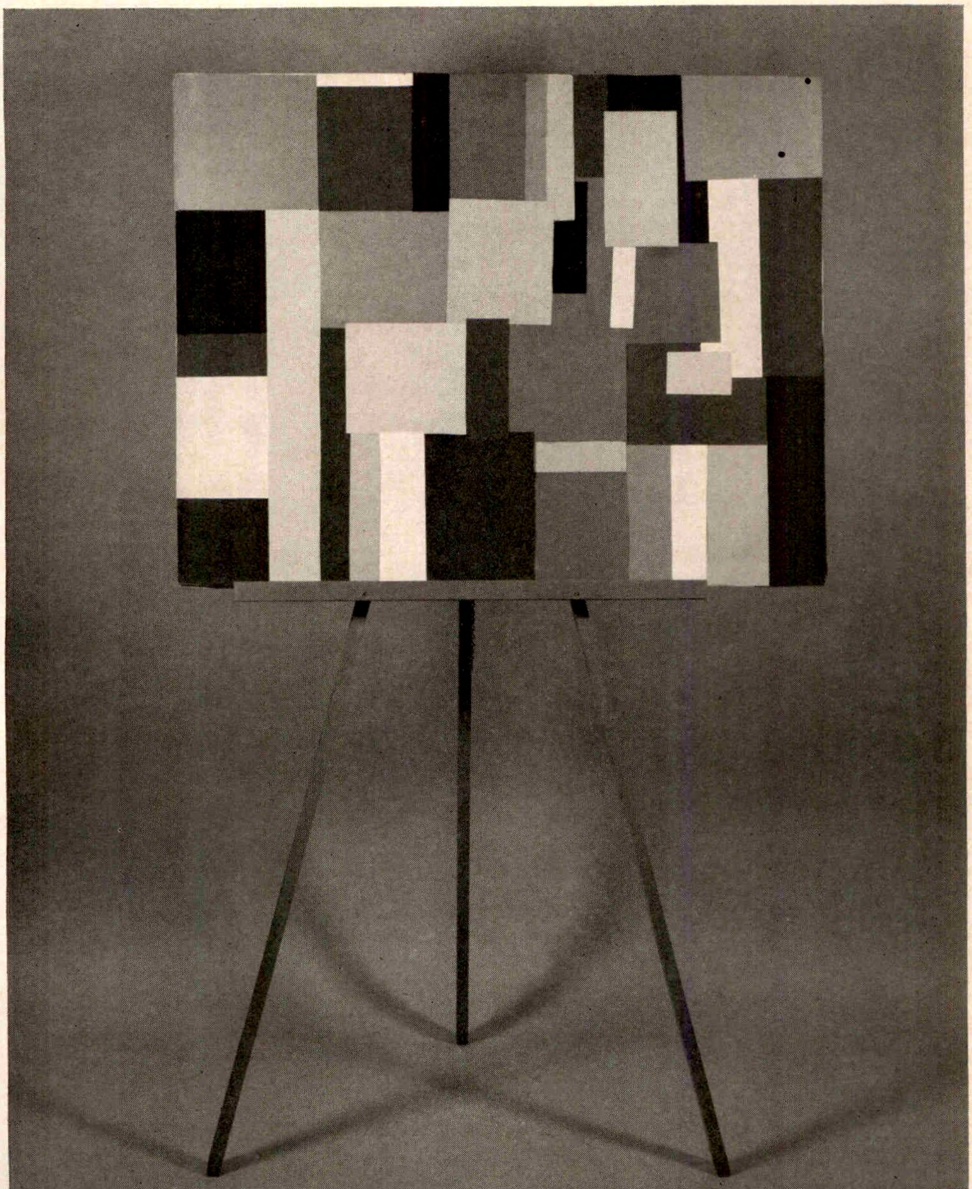

FIG. 1. The "Mondrian"

Before we leave this experiment, let us note that, whether the illumination is non-uniform or uniform, changes in the overall brightness of the projector do not alter the rank order of lightness of the various rectangles. Moreover, with or without mottle and wedges, the eye has the amazing ability to discern these lightnesses in flashes of light lasting as short a time as *one-millionth of a second*. In this fraction of an instant only an electron or proton can move, but our perception mechanisms need no longer period to record a lightness scale.

This whole series of experiments show that there need be no correlation at all between the amount of energy coming to the eye and the apparent position of a given object in the lightness scale. The approach which a physicist ordinarily takes in examining this problem is to illuminate a scene uniformly and then to measure the reflectance of objects in the scene—or, he may, so to speak, take the scene to pieces and pick little pieces of paper which he puts in a densitometer. In a sense, he uses a simplified "scene," in which he is comparing one object: the piece of paper, with another object: the controlled white chalk that he uses for his standard. The essence of his approach is to illuminate the chalk and to illuminate the piece of paper being examined with the same quantity of radiation and then to measure what returns from the chalk and what returns from the paper. He then assigns a reflectance number, a number which tells what per cent of the light which fell on the test paper returns from it. The great miracle of the eye is that it does not need what the physicist needs: it does not need uniform illumination in order to establish a lightness scale. In spite of surface lights, glancing lights, and scintillation lights, in spite of lights from building contours, in spite of an object's being surrounded by many light areas and many dark areas or by a motley group of light and dark areas, the eye can state, with almost unerring certainty, with an exposure of only a millionth of a second, a result which the physicist can find out only by eliminating all of the variables we have just named; the eye finds the reflectance that lies beneath all of these variegated superficial phenomena.

We now replace the Mondrian made of the various gray cards with a new display made by pasting designs cut out of *colored* papers on one extensive board (Fig. 2). The objects are randomly arranged to satisfy the following four conditions: (1) The objects overlap so that the support card does not show through gaps between them, in order to eliminate any common background. (2) In spite of their overlapping, the nature of each object is immediately perceptible so that, for reliable discussion from a distance, it can be remembered and identified by name. (3) Each object has in its immediate neighbors a large and unpredictable assortment of colors to eliminate any specifically colored surround. (4) Each object reflects not less than one-tenth of the light in those portions of the spectrum where the object has its least reflectance. This is to insure that

Figure 2: Color Cut-Out Board

surface light will be a small component of the measurement.

We use three illuminating projectors instead of the single projector that illuminated the Mondrian. Each projector has over its lens a band pass filter for one portion of the spectrum. The transmission curves for the filters are shown in Figure 3. The filters were chosen to isolate three *different* lightness scales by interaction with the absorption curves of our colored display. If the illuminating filters pass too broad a spectral band, then remnants of color remain in the objects viewed through

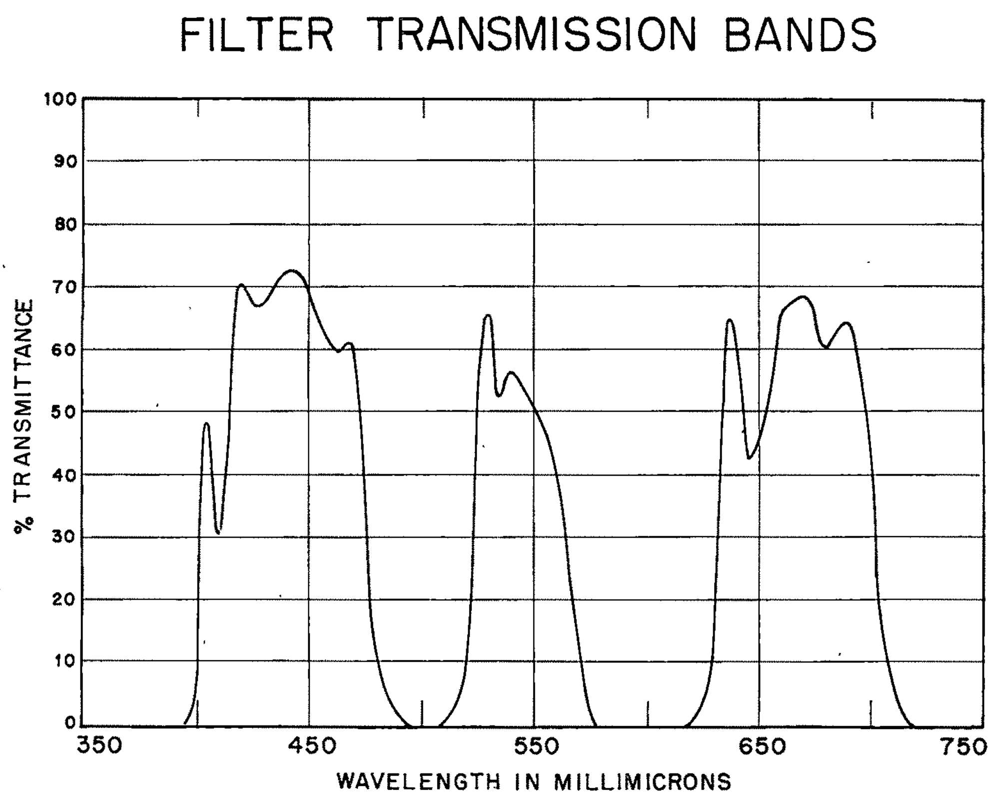

FIG. 3. Transmission Curves of Band Pass Filters used in the experiments with the Color Cut-Out Board.

them (for reasons that will be explained in our discussion of the applications of retinex theory). The spectral bands which these filters pass, however, are narrow enough so that, when the board of colored objects is seen illuminated with one projector and filter, the display is perceived as a nearly colorless pattern with a wash of colored light over it. A variable transformer is placed in the circuit of each of the lamps in the projectors in order to control each of their brightnesses. Each of the three slide holders is again available for inserting either wedges or mottle

slides. The projectors are now turned on one at a time, thus changing from one filter to another. With this change from one wave length band to another, the position of many objects in the lightness scale changes: lights become dark, darks become light, some mediums become dark, some mediums become light, some mediums stay medium, etc.

As the brightness of a *single* projector is altered while the others are still extinguished, one is not surprised to see that the lightness scale for that band of wave lengths, stays essentially constant. This corresponds with what we experience in everyday life. But one does not expect to see (except on the basis of our own earlier experiments) [3] that with all three projectors illuminating the board, the relative brightness of one of the projectors with relationship to the others can be varied enormously without profoundly altering the chromaticity of any of the patterns on the screen. We have used a number of test cards, all with random distribution of object colors, and all giving the same experimental result. As far as we know, only our retinex theory can explain this result, and it explains it very simply. Changing the brightness of one projector with relationship to the others, does not change the lightness scale for the retinex which responds to the wave lengths passed by the filter on that projector. Therefore, any alteration in the relative brightness of the three projectors does not produce an alteration in the relative rank order of lightness for a given object respectively on each of the three retinexes. Because the lightness scales are invariant with brightness, the perceived color must be invariant with brightness—if our doctrine is correct. (On the other hand, since the ratios of luminous energies at the three wave lengths are, by definition, a variant in this experiment, those ratios cannot be the determinant of color.)

We next introduced a mottle slide, like that used in the Mondrian experiment. This slide can be inserted in any one of the three projectors without significantly altering the color of the total display. Two of the projectors are turned off, leaving only the projector with the mottled display in it. As with the Mondrian, one finds very little difference in the lightness of each of the objects on the board from what it was without the mottle, despite the fact that the measured variation, because of the imposition of the mottle on the beam, may be large.

Again, one may illuminate the board successively with a variety of narrow bands of wave lengths. As long as the variation in the lightness scale for each projector remains trivial, a variation in *wave length* can be regarded as equivalent to a change in the *intensity* of that projector; as we have already seen, a change in over-all intensity does not change the chromaticity of our display.

Finally, we put a wedge, such as described in the Mondrian experiment, into one of the projectors, which caused a strong but uniform variation in the illumination across the scene. The two other projectors are for the

moment turned off, and, in the scene illuminated by the projector with the wedge in it, one finds many light objects which can be measured as sending to the eye as little light as do the perceptibly dark objects in the brightly lighted part of the scene. What happens when the other projectors are then turned on? They are illuminating the screen uniformly with their two bands of wave lengths. In classical theory, one would expect to find quite a new gamut of colors because of the new set of ratios of energies from various parts of the board. But one, indeed, finds that the introduction of the wedge into one of the projectors has altered the gamut of colors very little. In particular, one may amuse oneself by arranging wedges in the beams of the several projectors so that from a pear perceived as dark green on one side of the cut-out board and from an oak leaf perceived as bright brown on the other side, the eye is receiving the same number of quanta of the same frequency. (Remember that these objects have no predictable surround.) In other words, for the oak leaf and the pear, this is a null experiment in radiant input to the eye, with the two objects giving identical readings on examination with any sort of physical light meter. In spite of these identical inputs of radiation to the eye, the objects are perceived to differ completely in color.

Thus we find that color can hold its constancy while the ratio of energies varies in many ways and that another constancy appears: observers all see the same colors. Furthermore, we can assure you, that if in a dark room we were to turn the projectors on for only one-millionth of a second, one would essentially see the same colors even if the wedges or mottle slides were in the projector and even if the voltage of one projector were set in arbitrary relationship to the voltage of the other projectors. These experiments, then, urged us to seek a formulation which would permit the simultaneous and independent existence of three or more lightness scales, since three independent lightness scales are demanded by the observation that only perceived color and only perceived monochrome rank of lightness are invariant throughout our experimental variations.

Applications of Retinex Theory

Retinex theory can be applied to explain not only familiar experiences like the constancy of color during the change from blue sky light, to sun light, to tungsten light, and to even more extreme variations in the illumination, but also to explain a large family of impressive experiments. The results from such experiments include the extended gamut of colors produced in binary projection with assorted color filters. Equally important are other projections in which no color appears in spite of the presence of classical stimuli for variegated color. Since the explanation of the everyday experience of color constancy will become nearly obvious after we have discussed the other experiments, we shall leave the everyday situation for analysis later in this paper.

Projection with Red and White: The most dramatic and puzzling of the projection phenomena has been the richly colored image one perceives when two color separation black and white transparencies are superposed on the screen, one in white light and the other in red light. [4] The Maxwellian combination should be an assortment of pinks rather than the perceived grays, browns, whites, yellows, blue-greens, purples, and so on. Most efforts that have been made to explain this phenomenon have failed because the colors are fully seen in microsecond projection, because there are no predetermined color adjacencies in the projections, because there are no stringent requirements on the relative brightness of the two images, and because there is no need for uniform illumination of either of the component photographs. The essential invariance of the color throughout all these diverse conditions rules out all proposed explanations that involve fatigue, wandering of the eye, averaging over the picture, the effect of immediate surround, or, indeed, intelligible explanations in terms of the whole surround. The term "successive contrast" is ruled out by the microsecond flash, and the term "simultaneous contrast" offers no help in predicting what color will be seen in these images, in which the colors of the objects dispersed around a given object are random accidents of the environment. [5]

On the other hand, if we accept retinex theory and assume spectral sensitivities for the three retinexes corresponding respectively, for example, to the curves in Fig. 4 [6], then the colors perceived in any of the forementioned conditions as well as the invariance of the color over all of these conditions lend themselves to very simple explanation.

In red and white projection, the long-wave retinex system will form a lightness scale from the optical image of the black and white slide projected in *red* light. This perceived lightness scale will be invariant to changes in the brightness of the projector, to variations in the uniformity of illumination of that projector, and to reducing the time of projection to a microsecond. The other color separation slide, the one usually photographed through a green filter, is projected with *white* light. All three retinexes, the long-, the middle-, and the short-wave, will form lightness scales of this optical image. We can assume that these retinex images in terms of lightness will be like each other and that the objects within them will have the same rank order. If we now do our accounting of the images, we find: (1) a middle-wave record (that is, a photograph taken through a green filter) transformed into a lightness scale on the short-wave retinex; (2) a middle-wave record (that is, an identical photograph to that in (1)) transformed into a lightness scale on the middle-wave retinex; and (3) on the long-wave retinex, not only this same image but also the aforementioned long-wave image: these two will exist in combination. Let us pause to discuss this combination.

The essence of our argument up until now has been that the images

formed by the separate retinexes are not intermixed; that they are, in effect, orthogonal to each other as far as energy is concerned; that they are compared or correlated. When we come, however, to images that are superposed on the *same* retinex, then we are dealing with the simple addition of luminous energy. Thus, if the long- and middle-wave records are superposed on the long-wave retinex, then the lightness scale on that

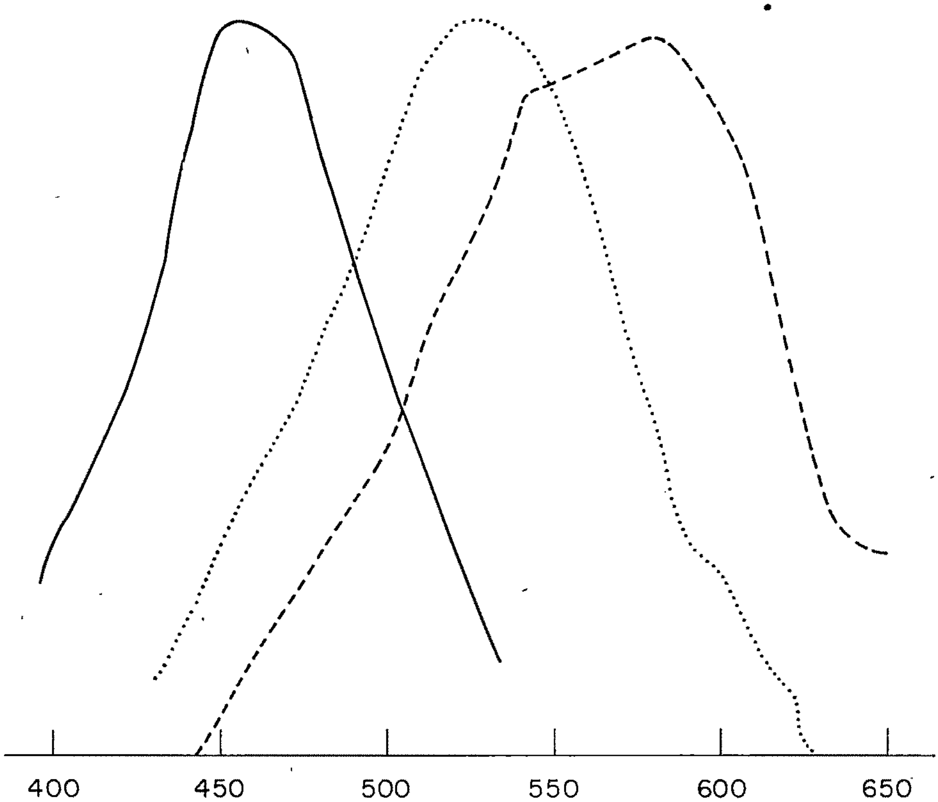

FIG. 4. Curve shapes suggested by the experiments of Marks, Dobelle and MacNichol for red, green and blue receptors.

retinex will be determined by the composite of the long- and the middle-wave optical image. The lightness scale formed from this composite is what would then be correlated with the other lightness scales. That is, the lightness scale on the short-wave retinex will be correlated with the composite on the long-wave retinex, as will, at the same time, the lightness scale on the middle-wave retinex. The lightness scale on the short-wave retinex is identical to the one on the middle-wave retinex in this experiment; there is a one-to-one correlation between them which would, therefore, not have to be considered in the determination of the colors

produced by the other two correlations. Returning for a moment to the composite image formed by the long-wave retinex, we note that, if we change the relative brightness of the red and white projectors, we *do* alter the composite rank order of lightness on this retinex, and, in particular, if we cut the brightness of the white projector to the point where its red component has, for example, one-half the energy that the long-wave record has, then the rank order will approach more closely to the rank order of the long record alone. But whether or not we change the brightness of the white projector, there still will be a pronounced difference in position on the lightness scale for each object in the composite on the one hand and for the same object on the middle and short retinexes on the other hand.

We can study the properties of some of these correlations by the following experiments: The long record is projected through a far long-wave red filter while the *middle* record is projected with a far *short-wave* blue filter. The image from either projector alone is perceived as being covered with a very desaturated over-all wash, with the objects seeming nearly colorless. When the two projectors are on, the objects become red, blue, brown, orange, and gray. If now the same experiment is repeated, replacing the far blue filter with a narrow band green filter, much the same gamut of colors is seen in the same places. except that many of the objects that appeared blue now appear green. In general, there seems to be a great similarity between the way the short-wave retinex behaves when its middle-wave image is correlated with that on the long-wave retinex and the way in which the middle-wave retinex behaves when the same middle-wave record is correlated with the lightness scale on the long-wave retinex. This similarity of the function of the short and middle retinexes would be lost sight of if one correlated a *short-wave* image on the short-wave retinex with the image on the long-wave retinex because of the great difference in rank order produced by illuminating the scene with short waves as opposed to middle waves. The engaging fact about red and white projection is that the *middle-wave* record is the one that is on both the middle and short retinexes, so that the small difference in behavior of the middle retinex and the short retinex manifests itself by the appearance in binary projection of both red objects on the one hand and blue or green objects on the other, depending on subtle differences in the absorption curve of these latter objects. Both the short and middle retinexes, because of their similarity in behavior, contribute together and without conflict to the sensations of grays, browns, oranges, and so on, when they are simultaneously correlated with the lightness scale of the long retinex. [7]

The variation of the relative brightness of red and white projection does not alter the rank order of lightness on the middle and short retinexes. If one does the arithmetic of addition of the images on the long

retinex, one finds a very slow and small change in rank order for large variations in the relative brightness. Here we see one reason for the extraordinary stability of the colors produced by the technique of red and white projection.

Projection with Red and Yellow: If one now examines projecting images with a red filter and a yellow filter in the way that Evans did in the early 1940's, one finds that, in that situation, one has on the long retinex the lightness scale produced by the composite optical images of the long and middle records, and, on the middle retinex, the results of the middle record, and on the short retinex, a weak middle record due to the crossover of the absorption curves of the middle photo receptors and the short photo receptors. Since a weak, but pure, optical record leads to the same rank order of lightness as a more intense record, the results from red and yellow will be essentially the same as from red and white.

Projection with Red and Green: Let us next consider projecting with red and green filters. [8] Here the long record is pure in its stimulus for the long retinex, the middle record is pure in its stimulus for the middle retinex, and the short and the long retinexes will reflect the crossover stimulus from the middle record. Again, the short retinex, having a pure image on it, will be forming the same lightness scale as the middle retinex. The crossover from the middle onto the long retinex will be of the magnitude always present in daily vision. The full rank order differences between the long and middle retinexes provide a very vivid picture, while the crossover of the middle record on the blue retinex provides some blue as well as green objects, as shown in the experiments above.

The Absence of Color: Three identical black and white images of a scene are prepared. One is placed in one projector; the other two are superimposed in registry and placed in the second projector. The single picture is projected with a red filter, the double with a green filter. The "red" and "green" images are registered on the screen. What appears to the observer is a uniform sepia hue everywhere in the image. Yet there is a whole family of ratios of energies of red and green radiation: Where the slide in the red projector passes one-fifth of the light, the slide in the green projector passes one-twenty-fifth; similarly, where the former passes one-tenth of the light, the latter passes one-hundredth, etc. [9] Classically, there is no explanation of the uniform hue which is observed. For retinex theory, however, the solution of the problem is simple: A moment's thought will show that superposing two identical images in the green projector has not altered the rank order of lightness as compared with the rank order of a single one of the two images. Therefore, there is a one-to-one correlation of rank orders between the middle retinex and the long retinex, and this should *not* give a variety of hues. By now it should be needless to say that variations in the relative overall brightness of the projectors does not alter the result.

Projection with Two "Narrow Bands" Close Together in Wave Length: If a middle record and short record photograph are projected through a sequence of interference filters on the two projectors, starting at 550 and 650 mμ and moving in until we reach the mercury yellow and sodium yellow lines—only 11 mμ apart, the colors keep their names but become less and less saturated. [10] (That they are still readily distinguishable at 578 and 589 mμ is dramatized by the reversal of the warm and cool colors upon interchanging the filters for the middle and short records.) If, as an exercise, we assume that with two bands close together in wave length, the short-wave record on the set of long receptors contains, for example, half as much energy as the long-wave record "on" the same set of receptors, and if we assume similarly that the long-wave record "on" the set of short receptors contains half as much energy as the short-wave record on the same set of receptors—then we can compute the change in relative rank order due to this crossover of spectral sensitivity. Picking many sets of rank orders, one finds only small changes in relative rank order, so that retinex theory demands that the hues stay essentially the same as the two wave lengths approach each other.

The World Seen Through a Filter: We have just described how slides taken with wave lengths far apart are seen as colored when viewed with wave lengths close together. If, however, the world is viewed through a single filter whose pass band is as wide as the separation in wave length of the previous two interference filters, then the world will appear *monochrome*. And it is not until the pass band of the single filter has been greatly widened that the world starts showing its gamut of colors. In retinex theory this is explained as follows: Because of the slow changes in the height of the absorption curves for most objects in the real world, the rank order does not change significantly until the pass bands of the filters are far apart—and, until then, the same lightness scale will be formed on all the retinexes when viewing the world with the filter. If, however, the same band pass—divided between two filters—is used to view two *pictures* (of the same subject) which already are markedly different in rank order, then even the small difference in wave length is, as we have seen, adequate to elicit two different rank orders of lightness from the retinex system.

The Coordinates for Color: In previous publications we have described our observations by a chart, the coordinate axes of which are log-percentage available short-wave energy and log-percentage available long-wave energy (Fig. 5). [11] This coordinate system can still be used with the mottle and wedge experiments described above, if we measure the reflectance at each point in our colored object board by comparing the reflectance of a piece of white paper with the colored paper at that point for each illuminating projector. In effect, this transforms the illumination to the equivalent of uniform illumination—the special case when rank

order of lightness equals measured rank order of reflectance. The coordinate system reveals the loci of warms, cools, neutrals, etc., in a colored scene. Furthermore, it emphasizes that color is determined by physically dimensionless terms. Nevertheless, this applicability of the coordinate system should not make us forget the miracle of the eye: the capacity to form the lightness scales with non-uniform illumination.

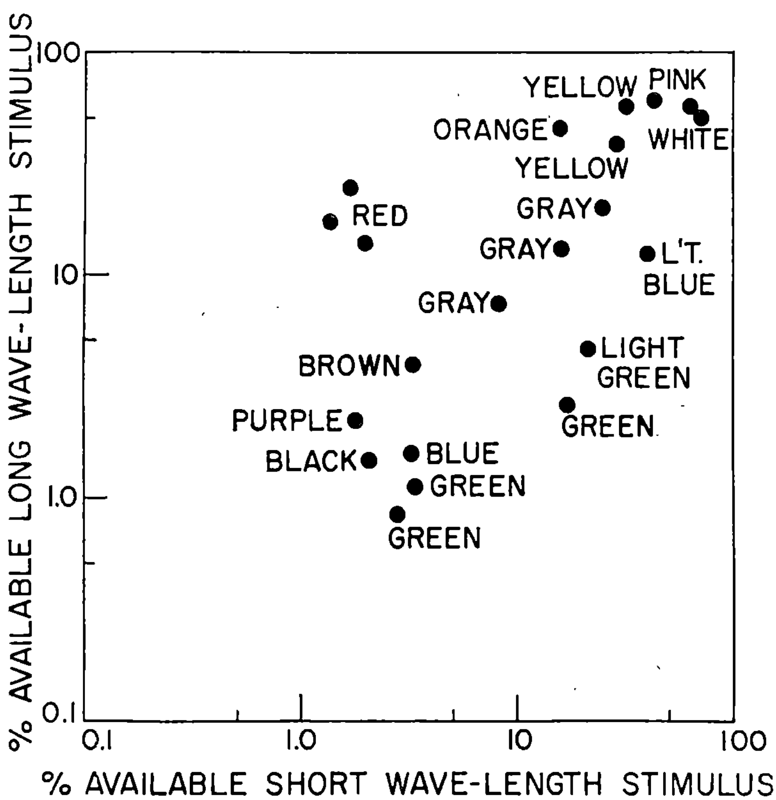

FIG. 5. The Coordinate System.

Color Constancy in Everyday Life: It is now clear that the changes from sky light to sun light or tungsten light are the kind of changes that would produce trivial variations in rank order on each of the retinexes and that the handling of color constancy in these situations is an easy task for the retinex system.

The Six Eyes of Man: In closing this talk we wish to open another subject: In stereo vision, images from the separate retinas are cerebrally compared. We find so many similarities between what this system will accept and reject and what the retinex system for color in the separate eyes will accept or reject, that it seems to us possible that the retinex

system for creating rank order of lightness may also be used as the basis for binocular vision. For the sets of "color eyes," differences in rank order of lightness are the source of color; for the stereo eyes, we suggest that *after* the lightness scales have been determined by the retinexes, the images are compared for those differences due to the separation of the left and right eyes. But all this is for future discussion.

"Reality" of the Retinexes: While we propose the retinex for its heuristic value in coordinating a large number of otherwise unconnected and inexplicable experimental observations, we must report our own quite human reaction that the retinexes have come to seem "real" to us. There is as yet no physiological basis for or against the existence of the retinex; how could there be until there was need to look for this type of integrating mechanism? Naturally, we hope that the contemporary vital and brilliant programs in visual neural physiology will bring support to our heuristic mechanism. But whether or not this physiologic support comes soon, we feel that there cannot be much difference between a "mental" reality which predicts a large body of results and a physical reality that has the same properties.

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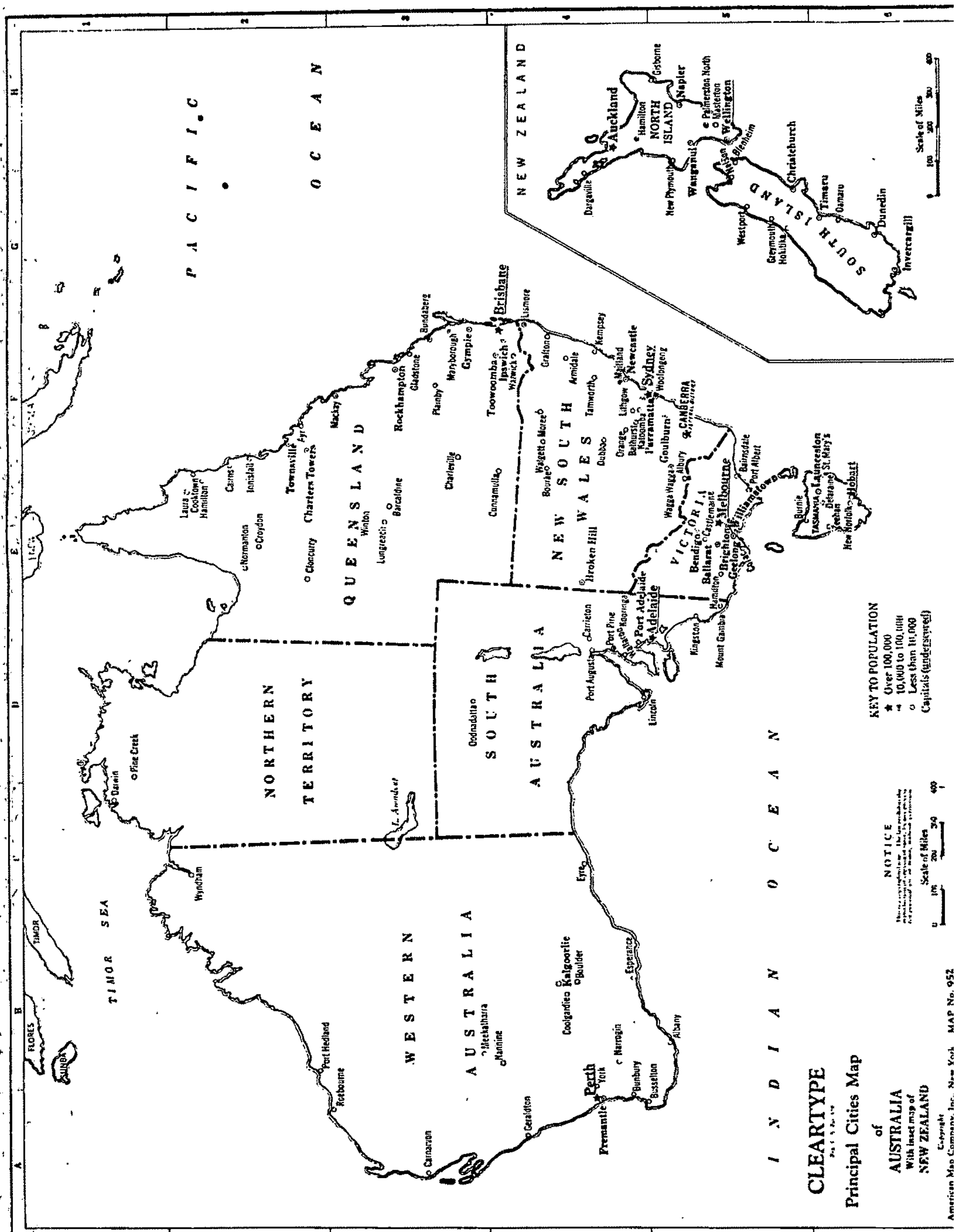
THE AUSTRALIAN RABBIT

By JEAN M. INGERSOLL

ORIGINATING in the Mediterranean basin, the European wild rabbit *Oryctolaqus cuniculus* has been introduced to most regions of the world, and, for the most part, has fitted into its new environment as a domesticated animal, a source of food and clothing for man. However, some eras and areas have known it as a pest; the Romans introduced the species to the Balearic Islands from Spain, where the rabbits became so numerous and did so much damage by undermining buildings that the inhabitants sent a deputation to Rome asking for help in ridding the islands of them [1]. In this century, it has become famous as a pest to Australians who thought to provide themselves with an inexpensive, readily available food supply but found these advantages offset when the rabbit proved to be an uncontrollable competitor for their most valuable crops and a despoiler of the land itself.

The economic costs (and emotional response) of the introduction to the Australian continent of this animal species are indicated in a 1928 report issued by the Department of Agriculture of New South Wales. "... no single circumstance in the history of Australia has so profoundly affected the economic development of our pastoral and agricultural industries as the introduction of the European wild rabbit. The cost of the rabbit, even to the State of New South Wales in the forty-five years that have elapsed since the animal first began to be reckoned a serious pest, is beyond all calculation. Without taking into consideration the millions of pounds sterling which have been expended in fencing, netting, poisoning, fumigating, digging out, and other direct methods of prevention and destruction, the cost to the country in loss of pastures and their deterioration in value, with the consequent loss of output of wool, livestock, and fodder crops certainly runs into many scores of millions of pounds [2]."

The first account of rabbits in Australia is a livestock report of 1788 for the settlement of Port Jackson which listed five rabbits kept as pets by their owners. There were several later introductions of rabbits into New South Wales, Victoria, Tasmania, and other small islands in the Bass Strait, these latter being stocked "for the benefit of unfortunate voyagers who might be thrown hungry ashore in this locality," but, because of dense bush and mountain barriers, the rabbits of the early continental introductions on the east coast of New South Wales appear not to have spread inland [3]. It was not until 1859 that the "rabbit menace" of Australia developed. Twenty-four wild rabbits were brought from Europe and delivered to Mr. Thomas Austin of Barwon Park,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Principal Cities Map
of
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With inset map of
NEW ZEALAND
MAP No. 952
KEY TO POPULATION
Over 100,000
10,000 to 100,000
Less than 10,000
Capitals (underscored)
PACIFIC OCEAN
INDIAN OCEAN
TIMOR SEA
NORTHERN TERRITORY
QUEENSLAND
NEW SOUTH WALES
VICTORIA
SOUTH AUSTRALIA
WESTERN AUSTRALIA
NEW ZEALAND
NORTH ISLAND
SOUTH ISLAND
Brisbane
Sydney
CANBERRA
Melbourne
Adelaide
Perth
Hobart
Darwin
Auckland
Wellington
Christchurch
Dunedin
Invercargill

near Geelong on the southern coast of Victoria. Mr. Austin released thirteen of the rabbits on his estate that year. Three years later they were looked on as pests, and six years later Mr. Austin reported that he had killed 20,000 rabbits on his estate and believed that there were at least 10,000 remaining.

From Barwon Park, the rabbits spread quickly over the western section of New South Wales, for geological conditions in this region—long stretches of sandy soil—were ideal. From their stronghold in western New South Wales the rabbits rapidly spread out through the entire state and invaded a large part of Queensland, all of Victoria and South Australia, considerable areas of Western Australia, and even some areas on the north coast. By 1928, the rabbits had spread over nearly two-thirds of the Australian continent [4]. Besides finding soil ideal for burrowing and a plentiful food supply, an important factor favoring the early rapid increase in the rabbit population was the absence of several important natural enemies [5].

Early Control Measures

In 1880, the rabbit problem was recognized to be beyond the control efforts of individual property owners and a series of legislative enactments followed. The Rabbit Nuisance Act of 1883 empowered the government to "enter upon all lands to enforce rabbit destruction," and a bounty program was instituted. Seven years later, the government had paid more than $7,275,000 for rabbit scalps, but the expenditure proved to be useless in the fight to control the animal. Other legislative acts provided easy repayment terms to "necessitous settlers" for rabbit-proof wire netting, again with little effect on the problem [6]. Various methods of control were attempted, including trapping, poisoning of water holes, fumigation and ripping-out of warrens, and the construction of miles of fencing, but it was not until the rabbits were innoculated with a virus disease, myxomatosis, in 1950, that significant reduction of the hordes of wild animals was effected. Unfortunately, the seemingly fatal blow to the rabbit population of this disease is now known to have been but a temporary setback.

Present Problem

Destruction of Vegetation: Rabbits prefer the most succulent plants and in pastures attack first the young shoots of the clovers. Their scissor-like teeth enable them to nibble the entire plant down to the soil surface, and the animals will grub roots out as well. They also kill trees by ring-barking them [7]. The result is the creation of acres of barren, erosion-inviting prairie and "jumping" sandhills, with consequent loss of grain production and pasturage for sheep and cattle. Although there was a considerable amount of arid land on the continent before the introduction of the rabbit, the animal is charged to a great degree with creating Aus-

tralia's present dust bowl problem [8]. Photographs made with a telescopic lens showing hundreds of rabbits around a water hole in a sandy wasteland illustrate their numbers and the damage done to the land. A photograph showing a lush wheat crop growing within a rabbit-proof fenced area contrasted with the barren land outside the protected enclosure is a striking demonstration of the thoroughness with which vegetation is destroyed by the animals as well as of the usefulness of effective fencing.

Loss of Income from Livestock: In 1953, the Minister in charge of the Commonwealth Scientific and Industrial Research Organization (C.S.-I.R.O.) estimated that there are between 500 million and 1,000 million adult rabbits in Australia in an average year over an area of about 1,000,000 square miles, with the density varying between small numbers that make them something of a local curiosity to "being so thick that you have to kick them out of your way" [9]. Based on the assumption that seven or eight rabbits eat about the same amount of pasturage as one sheep, it was estimated that if there were no rabbits in Australia the land could support an additional 100 million sheep, or the equivalent in cattle, and the increased income to Australian economy from wool and beef would amount to about $896 million a year. Rabbit skins and meat are also exported, the income from them amounting to $13 million in 1953, but such an insignificant sum is counted as token salvage from heavy losses in land, livestock, and agricultural products [10].

Natural History

Belonging to the order *Rodentia,* the rabbit is of the same family as the hare but is distinguished from it by its smaller size, shorter ears and feet, by the facts that the young are born naked and blind and that it lives in burrows undergound. Members of the family *Leporidae* native to North and South America are hares, not rabbits, and dig but little [11]. *Oryctolaqus cuniculus* has been imported from Europe to the Americas for use in laboratory experiments, and it was from diseased laboratory rabbits in South America that the deadly virus used in biological control of the Australian rabbit was first recovered.

In favorable circumstances a doe will breed at four months and will have six litters a year, averaging six kittens to the litter. Within twelve days the young rabbits leave the nest to feed outside the burrow, and, at the age of three weeks, are independent of the mother. Computations have been made, based on average-sized litters (and assuming, of course, that all progeny live and reproduce in the stated time period), resulting in a theoretical total in three years of 13,715,000 offspring from a single pair of rabbits [12]. Mortality of the young is estimated to be about 80 per cent, and the average life span of the adult is one year [13].

The rabbit has adapted to varying environmental conditions in

Australia; when driven from pasture land and the vegetation it prefers, it retreats to timbered and rocky hill country until the population builds up such numbers that it is forced to reinvade open country [14].

Biological Control Measures

Natural Enemies: The absence in Australia of important natural enemies of the rabbit, the mongooses, stoats, and weasels, is one ecological factor accounting for the wildfire spread of the rabbit over the continent. Introduction of these and other predators to Australia has often been suggested but is opposed on the grounds that the danger of their preying on useful wildlife and domestic poultry is a more likely result than successful control of the rabbit; introduced foxes and dogs allowed to run wild were expected to subsist on rabbits, but they have caused enormous losses among new-born lambs and ground-nesting wild fowl and have not brought about a significant decrease in the rabbit population. Some natural control is exercised by the dingo and certain native cats (*Dasyures*) among the mammals, and by the eagle hawk [15].

Disease: Disease has long been recognized as an important factor in reducing the rabbit population. Early investigations found several principal disorders –the Bladder Worm, causing rabbit hydatids, Coccidiosis, a parasitic disease, and a disease caused by a microorganism known as the "yalgogrin microbe [16]." Although these organisms caused periodic epidemics, recovery of the rabbit population was always rapid.

In 1887, the New South Wales Government offered $121,250 reward for the demonstration of an effective method of exterminating rabbits. In Europe, Pasteur had spread a culture of chicken cholera bacteria around the burrows of rabbits on the estate of Mme Pommery, owner of the famous champagne firm; within a fortnight all the rabbits were dead. Interested in the Australian problem, Pasteur sent an assistant to New South Wales to attempt distribution of chicken cholera bacteria, but the government decided against introduction of the disease because of its pathogenicity for domestic fowl and other animals [17].

A bacteriologist, G. Sanarelli, working in Uruguay, in 1898 recovered a virus from diseased laboratory rabbits which he named infectious myxomatosis. In 1927, Professor H. B. Aragao of Brazil suggested that the virus might be used for the destruction of rabbits in Australia, but it was not until field trials and laboratory experiments in England and Australia had established that the virus combined a high mortality rate for rabbits with a high degree of host specificity, so that other valuable species of animals would be unaffected, that a serious attempt to introduce the disease to Australia was made. In 1950 and 1951 rabbits were innoculated with the virus in several areas of the States of South Australia, Victoria, and New South Wales. From these foci the disease spread rapidly by direct contact and by mosquito and flea vectors; the

resulting epizootic killed rabbits by the millions. Mortality is estimated to have been from 80–90 per cent of the rabbit population. By 1953, it was thought that the rabbit menace had been virtually eliminated [18]. Now, however, the period of large-scale epizootic reduction of the rabbit infestation of the early 1950's is viewed as an invaluable breathing space rather than the end of the problem, and the subsequent low level of infestation is accounted for by a combination of factors, not the virulence of myxomatosis alone. Unfavorable weather for breeding is considered by an official of the C.S.I.R.O. to be as much responsible for the low numbers of rabbits in the period 1953–59 as myxomatosis [19].

Decreased efficacy of the virus after the dramatic decimation of the rabbits in 1950–51 is attributable to familiar biological interactions of disease-producing organism and host—usually an eventual equilibrium is reached so that both continue to survive. There has been an increase in the number of rabbits in the past few years, and it is believed that the population is building up from individuals that have acquired immunity to myxomatosis. In some districts this development of immunity has caused a fall in the death rate over a period of seven years of from 90 to 25 per cent. Recent research has shown that genetic changes in the virus itself have resulted in the evolution of strains considerably less virulent to rabbits [20].

Mechanical Control Measures

Fencing: By 1901, the rabbit was beginning to invade wheat farming regions in the West despite the desert of Nullarbor Plains, which until then had been considered a natural barrier between West Australia and the infested States to the east. Work was begun by the State of West Australia on what is perhaps the longest unbroken fence in the world. The fence was started at a point on the southern coast, but while construction was going on rabbits were found to have already gone past the intended barrier to the north, and a second fence, 75 miles west of the first, was started at the break-through point, with construction going north and south simultaneously. This fence was brought to the sea at the southwest coast, and another section added from the north end, reaching the sea on the west coast. The western wheat belt was thus enclosed by more than 2000 miles of fencing, which cost over $1,000,000. The fence was maintained in the early years by camel-mounted patrol officers, each being responsible for about a hundred miles of fencing [21]. In the late 1950's, it was decided that the 500-mile northern section of the fence was no longer needed, but the western and southern fences (about 1522 miles) are still maintained [22]. While the fence has not prevented the rabbit from entering West Australia to become a serious pest in some areas, it has apparently helped considerably in keeping the numbers low and has been a noticeable check to the great waves of

migrating rabbits in seasons when the animal found conditions unfavorable in the eastern states [23].

This kind of large-scale fence building is not likely to be repeated; most authorities believe netting in (wire-mesh fencing) of individual holdings together with cooperative efforts by the government to control the rabbit on public lands is more economical and more effective.

Destruction of Harbourage: Eliminating rabbit harbourage, both surface and underground, is often effective in ridding areas of rabbits. Hollow logs, piles of debris, and heavy undergrowth, which may provide homes for the rabbits on the surface, are destroyed. Underground burrows are ripped open by pick and shovel, or by tractor where the infestation is extensive. Dogs are used to locate warrens and to drive the rabbits into their burrows where they can be sealed in and killed by fumigation.

Trapping: Various kinds of traps have been used against the rabbit, among them steel spring traps, wire netting traps at the mouth of the burrows, pit traps, and water traps—sometimes containing poisoned water. These latter are said to be especially effective when migrating rabbits are moving in "waves," and have been used chiefly in the far west, and in drought time. When rabbits were moving from west to east during a drought in 1906, it was reported by the manager of a ranch at Boorooma that as many as 100,000 per night were destroyed by this means [24].

Rabbit Drives: Community rabbit drives have been employed, with the animals set into panic by the barking of dogs, cracking of whips, shouting, and beating of tin pans until thousands of rabbits joined the stampede and were driven into fenced killing yards. They were looked on as a method for the most part useful only at the beginning of a determined program for eradication that would utilize procedures of greater efficacy, such as continual destruction of harbourage and chemical controls.

With the recognition that control of the rabbit by myxomatosis is decreasing and that "they will never see the end of the rabbit," officials are trying to gain the cooperation of individual landowners in the resumption of some of these older methods of mechanical control—"digging and dogging," and the placing of steel traps.

Chemical: Phosphorus, strychnine, and arsenic have been used in baits and in poisoning water holes to destroy millions of the rodents in Australia, but unfortunately not only rabbits but considerable numbers of other wildlife and livestock have been killed by this method.

Fumigation of warrens was one of the first control methods used, usually with carbon bisulphide, calcium cyanide, or carbon monoxide. Calcium cyanide is more effective than carbon bisulfide, more economical than carbon monoxide to distribute, and is much less dangerous to handle than strychnine or arsenic. In flake or dust form it is blown by machine into rabbit burrows, which are then sealed off. On exposure to

air the calcium cyanide releases hydrocyanic acid gas which coats the walls of the warren and remains lethal for a number of hours.

New poisons, and new techniques for effective distribution of them based on ecological studies of the rabbit, have recently been developed. Experiments with "1080" (sodium fluoracetate) in the past year indicate that this poison, properly used, achieves a 98 per cent kill, and a program for training rabbit control inspectors in the use of 1080 is being developed [25]. Fluoroleic acid, alleged to be the active ingredient in ground-up seeds of *Dichapetalum toxicarium*, is a humane poison causing death after a single heart attack, but use of it is limited because it is also very toxic to sheep [26]. A private concern has reported the development in the past year of a new technique of fumigation, using carbon monoxide and a foaming agent pumped into warrens under pressure. The foam remains stable up to three days and leaves a tacky residue which discourages recolonization of the burrow. The cost is said to be the same as conventional poisoning and fumigation methods with no attendant danger to humans or livestock [27].

Outlook

Educational and research emphasis by authorities responsible for control of the rabbit, still rated as the most serious of pests in Australia, is on the need to capitalize on the reduction of the rabbit population caused by myxomatosis in the past decade in order to keep the population at its present relatively low level. An intensive program combining old methods of digging and fumigation and the use of chemicals, principally the poison 1080, is considered essential if the rabbit menace is to be prevented from recurring in pre-1951 dimensions. Ecological studies now in progress are expected to be of value to control programs utilizing combinations of biological, chemical, and mechanical methods by contributing information on fluctuations in populations, reproduction rate, infestation with parasites, social behavior, and adaptability to different habitats. Such studies have already been useful in poisoning campaigns taking advantage of territorialism in rabbit behavior, so that poison bait is placed where it is most likely to be taken by the animal.

The control program outlined above depends for its success on the consistent cooperation of individuals and government agencies; and the continued effectiveness of myxomatosis. Two possible ways to augment the mortality of rabbits from this disease and to overcome the factor of developed immunity have been suggested: successive widespread reintroductions of the fully virulent virus form which would kill rabbits that have not developed immunity, and the isolation of a mutant of the virus which would be unaffected by the presence of antibodies to the original virulent form, thus producing fatalities in "immune" rabbits [28].

It is recognized today that eradication from Australia of this intro-

duced pest may not be possible, and at the present time the outcome of attempts to control it is unpredictable. However, successful control of the European wild rabbit is essential to Australia not only for the immediate economic advantages of maintaining good quality pasturage for livestock and the protection of agricultural crops, but for conservation of the land itself. Flood control and recovery of eroded land for agriculture require the cultivation of soil-holding vegetation. Rabbits have destroyed such vegetation over large areas of Australia. Fully effective countermeasures to the rabbit "plague" will serve both the short-term purpose of increased income from the land and the long-term one of soil conservation.

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PERSPECTIVES

SCIENCE IN RUSSIAN CULTURE

By JAMES H. BILLINGTON

THE ROLE of science in Russian culture is one of the most fascinating and important subjects in the disturbed history of that country. The accomplishments of Russian science helped change the world balance of power in the fifties. The attitudes of Russia's scientifically-educated elite will, in the sixties, help determine the future of Russia—and of a Communist world very much in ferment.

It is disappointing, therefore, that there should be so little written on the development of Russian science. Russian historians moved from general neglect of scientific developments in the pre-revolutionary period to chauvinistic hagiography of early Russian accomplishments during the Soviet period. Western students of the USSR have also oscillated between the belief current in the high Stalin era that science was dying out to the uneasy fear in the space age that science may be flowering in Russia as in no other country. One is left with the disquieting feeling that what was undervalued before is overvalued now. Despite the balanced expositions offered by articulate American scientists of Russian extraction such as John Turkevich, even informed Americans tend to be bewildered by the spectacle of a nation backward in so many respects suddenly attaining such spectacular accomplishments in sophisticated fields of science and technology.

Part of the problem lies in the cultural and linguistic provincialism of Western scientists and historians of science. Only in the 1930's did the English-speaking world become aware of the unique, 50-year-old Russian tradition of soil studies which is designated by the untranslatable term *pochvovedenie*. This science revolutionized soil study by turning attention away from purely geological factors to the many-sided dynamics of soil formation; and has had important practical results in a whole range of areas (such as permafrost research) in which the Soviet Union enjoys world renown. Only now, in the wake of Soviet space accomplishments, is the Western world becoming aware of the remarkable late 19th century work on rocket propulsion (of both a theoretical and experimental nature) done by Constantine Tsiolkovsky.

Even when such accomplishments are recognized—and when one admits to the pantheon of world science such men as Lomonosov (the 18th century father of physical chemistry), Lobachevsky (the

early 19th century expositor of non-Euclidean geometry), and Mendeleev (the codifier of the periodic table)—one is still left with the feeling that these were brilliant exceptions to a prevailing rule of superstition and sloth.

The publication of the first of a promised two-volume history, *Science in Russian Culture* (Stanford, 1963) by a California sociologist, Alexander Vucinich, adds to the picture by carefully chronicling the institutional structure through which Western-type scientific activity developed in Russia up to 1860. One is reminded that the Russian Academy of Sciences was founded in 1725, only about a half century after those of England and France; that prominent figures from the newly-founded Berlin Academy helped draw up the plans; and that the most prominent of all, Leonard Euler, was attracted to Petersburg for the last and most productive seventeen years of his life. One is also reminded that, by the 1830's, Russia possessed the best equipped astronomical observatory in the world at Pulkovo near Petersburg and a vigorous astronomical tradition (taken over from the Baltic German principalities, which already had a strong tradition in the mid-17th century, when the observatory at Danzig was the best equipped in the world).

Vucinich's rich inventory of personalities and his enormous bibliography on the history of Russian science adds, however, more to information than to understanding. He confines himself largely to a sociological description of how the scientific attitude grew and was integrated into Russian culture. The casual reader may be left with the conclusion which is to some extent already implicit in such an approach: That science was growing and being integrated gradually and peacefully—just a bit tardily by Western standards.

The fact remains, however, that scientific activity prior to the mid-19th century was that of an essentially foreign tradition groping for roots in unfriendly soil. Indeed, the evidence which Vucinich conscientiously presents supports the very conclusion which he found "hard to accept" in the preface: That prior to the last century science was "a rationalist aberration operating on the fringes of Russian culture." To deepen understanding of the "cultural integration of science" one must consider the peculiar nature of native Russian culture and of the philosophical path through which scientific thought reached Russia. Vucinich's intellectually provincial (and ultimately unscientific) dismissal of most native Russian thought as a formless sea of "mysticism" and "irrationalist (sic) metaphysics" must be overcome—if only to answer two searching questions which the author himself poses in his preface: (1) How can one explain in such a land the remarkable scientific accomplishments of the last 150 years? and (2) How, moreover, can one explain the wholesale acceptance of Marxist scientism, "the *reductio ad absurdum* of rationalism" as the basis for an entire culture?

1. Why the sudden flowering? An abrupt flurry of scientific and technological accomplishment does not, of course, necessarily require a long preparatory build-up—as the experience of modern Japan and other rapidly developing nations has recently shown. Since, however, Russia was exposed to the West over a long period of time and experienced a gradual build-up prior to the "take-off" of the 19th century, one cannot speak of the stimulative shock of sudden contact with a new mode of thought. Rather one must consider the role of traditions other than those of the Westward-looking, German-dominated academies and universities.

Long before the belated appearance of these formal institutions of learning in the 18th century, the word "science" (*nauka*) was widely used—generally in the sense of "skilled technique" rather than theoretical knowledge. Derived from the word *nauchat*, (to teach), this "science" of early modern Russia was an impressive body of practical skills transmitted orally and designed to serve the purposes of a given body of craftsmen. By the end of the 16th century, Russia had built the largest bell and the largest cannon in the world without any apparent understanding of the principles involved. Some of the most elaborate machinery in the 17th century world was operated and serviced in Russia (such as the giant Kremlin clock with a rotating face). Some of the mechanical devices apparently in use throughout this period in the distant northern monastery of Solovetsk reveal technological skills quite equal to anything in the contemporary West. Yet not even music was systematically transcribed until the late 17th century. Only in the richly-documented history of early Russian medicine does one find a continuing *written* literature of some interest from Nicholas of Lübeck in the late 15th century to Tveritinov in the early 18th.

In the light of this tradition, Peter the Great's effort to produce "science by decree" seems somewhat less revolutionary than historians of science may be inclined to assume. For Peter was concerned solely with science in the old sense of technological skill. Indeed, the main scientific accomplishments in 18th century Russia came in the response of visiting foreigners to more difficult technological questions. The problems of mapping and surveying the empire led to the more sophisticated expeditions of Pallas and the development of geodesic and ethnographic research in which Russians still excel; and the problems of efficiently moving and arranging troops led to some of Euler's most important mathematical innovations.

This tradition of an essentially practical science—one which asked no more questions of the material world than were needed to deal with the concrete concerns of the moment—seemed to many Russian thinkers in the past century a source of great native strength. Shchapov, one of the best early students of Russian intellectual development, predicted in the

1860's that as the long-neglected theoretical side of Russian science develops, it will not divorce itself from concern for practical applications as in the west. In Russia, he insisted:

. . . science and life, knowledge and labor, practice and theory will go hand in hand. Science will not be abstracted from life, but a vital guiding force . . . the scientifically-working intellectual class will not be a cast, a guild, a minority cut off from the people, but an all-people's intelligentsia . . . a thinking and working head for the social organism . . .

Here we see already a utopian and at the same time authoritarian view of the role of science which anticipates that of official Soviet ideologists and leads naturally to Vucinich's second question:

2. Why Marxist scientism? One cannot beg this question by answering that it just happened to be the outlook of Lenin, for the prior question remains: How did Lenin's ideas happen to strike such a responsive chord in so many Russian imaginations during the chaotic years of revolution and civil war? Nor will the details of increasing scientific activity in themselves explain anything, for scientism relates to the world of commitment and belief rather than that of hypothesis and experiment.

One simply cannot escape the basic fact that culture in Russia was dominated by religion for a deeper and longer period of time than in the West, and by a form of Christianity that was—compared to that of the West—both more ornately majestic in worship and more profoundly anti-rational in dogma. Not only Shchapov and Chernyshevsky in the 19th century, but Stalin and Molotov in the 20th were ex-seminarians. The radical tradition of the late imperial period grew up with a faith in what science could do in society that was unaccompanied by any first-hand experience of what scientists actually did in laboratories. Thus, at the simplest level, the sudden adoption of Marxist scientism as the basis for an entire culture represents simply the extension to society of what the pessimistic late 19th century religious thinker, Vladimir Solov'ev, referred to as the "exchange of catechisms" by the intellectuals.

However, this revolutionary transition from ritualistic theology to rationalistic scientism also has interesting origins in the unsettling confrontations with Western modes of thought during the early modern period. The Latinized scholasticism that largely replaced traditional Orthodox thought in the theological academies of the 18th century was not simply a hindrance to scientific development, but an important factor in conditioning the Russian aristocracy to think in abstract, universal terms.

Even more fateful was the influx of German pietism which dominated the thinking of Alexander I and of most of the numerous higher educational institutions founded during his reign. Anti-scholastic pietists helped lay the basis for a genuine scientific tradition in the Russian

academies and universities somewhat as the Franciscan revolt against Aristotelianism can be said to have done in the early life of universities in the West. Like the Franciscans, the pietists mixed in with their science a kind of pantheistic mysticism which fitted in with a strong tradition of occult thought.

The earliest Russian diplomats in the late 15th century tended to look not so much for detailed political or technical information about the west, but rather for the alchemistic or astrological texts which would somehow explain to the tsar the "secret" of the West. The famed "Judaizing heresy" within the Russian court at the end of the century was propagated by Russian diplomats recently returned from the West, by foreign doctors resident in Russia, and possibly also by Jewish kabbalists. Emissaries of Boris Godunov tried to get the Cambridge spiritualist and mathematician, John Dee, to set up a school in Russia at the beginning of the 17th century; and nowhere did the cosmic theosophy of Jacob Boehme find a greater reception than in Russia.

Thus, against the extremely anti-rational, historical theology of Russian Orthodoxy, there grew up a rival—albeit suppressed and persecuted—tradition of anti-historical rationalism in early modern Russia. Anti-trinitarian Socinianism was, after all, a Polish movement that found its deepest roots in White Russian regions gradually incorporated into the Romanov empire. From the time of the church council of 1553 condemning White Russian heretics under Ivan the Terrible, northern monasteries became prisons for ideological dissenters. The Russian prince, Khvorostinin, was incarcerated for converting to Socinianism during the Time of Troubles; and a relative of Abraham Palitsyn (the monastic leader of the nationalist-Orthodox revival of the 17th century) became one of the first of the long line of ideological dissenters to be sent to Siberia for his rationalistic beliefs.

Faced with such tribulations, Russian rationalism (unlike that of the West) tended to assume the form of a fanatical religion—rather like the one which persecuted it. The path to Marxist scientism was prepared by the successive infatuation of 19th century Russian intellectuals with Schelling, Hegel, and Comte—all figures offering an all-encompassing philosophy which encouraged rational science and at the same time sought to provide a systematic philosophy of history to displace the discredited one of Russian Orthodoxy. It seems appropriate—and perhaps prophetic—that the packet of books sent to Russian missionaries in China in 1734 should include a volume of Newton, and that the book should be not the *Principia* but the *Chronology of Ancient Kingdoms Amended.*

Russian science tended to be Gnostic rather than agnostic as in France and England. Failure to appreciate this important philosophic fact leads most students of Russian thought to misrepresent drastically a

figure like Nicholas Novikov, the most important single propagator and popularizer of knowledge in the late 18th century. His "mysticism," far from being an irrelevant psychological aberration, provided the very basis for his intoxicating faith in science. Science was for him absolute knowledge of the inner secrets of the universe—rather in the Neo-Platonic tradition of the high renaissance. He was a passionate convert to Rosicrucianism, and viewed his popularization of scientific knowledge as a necessary prerequisite for the higher pursuits of full knowledge and moral perfection. He introduced the word *sientificheskii* into Russian to describe the secret circle of intellectual leaders to which these pursuits should be confined. Although this word never replaced the term *nauchnyi* in ordinary Russian speech, much of its occult significance was transferred to the concept of "science" in the late imperial period.

With the spread of enlightenment by the followers of Novikov and the establishment of a university system dominated by German idealistic philosophy, the way was prepared in the early 19th century for some strikingly original theories ranging from Lobachevsky's prophetic, non-Euclidean geometry to Odoevsky's fantastic "theosophic physics." The ferocious materialism and physiological determinism of the Sechenov-Pavlov school, which has provided much of the basis for Soviet science and scientism, was both a reaction to the idealistic science of the first half of the century and a logical outgrowth of it. For it was but a short step from the intoxicating assertion that God was everywhere in general to the liberating discovery that He was nowhere in particular.

Thus, there appear to be historical roots for some of the perplexing present-day facts about the "cultural integration of science" in the USSR. The desire to make science a religion if religion was to be overthrown was present in the earliest Muscovite heresies. The desire for concrete and imposing technological results is constant from the successful effort in the 16th century to "overtake and surpass the West" in artillery. The dependence of Russian science on stimulus from abroad (and from partly assimilated Jews and Germans within) has traditionally been of great importance, and is contributing now to the pressures for greater foreign contact and inner freedom which are such an important part of the ideological ferment in the USSR today.

Those aware of this latter tradition are not likely to be so surprised that the unprecedented growth of scientific and technical education in the USSR—far from producing a generation of technological zombies—seems rather to be infecting Soviet society as a whole with a kind of creeping pragmatism.

Indeed, some of the Russian scientists of the past like Lomonosov and Mendeleev had a sensible and constructive interest in social questions and a profound, yet practical, concern for broadening the range of free inquiry. It may not be too much to hope that this tradition will yet

prevail; and that the catechistic scientism of Marxism-Leninism will yet be swept aside by those like the lonely inventor in Dudintsev's *Not by Bread Alone,* who affirms that "Once a man has started to think, he cannot indefinitely be denied his freedom."

GRANTS-IN-AID OF RESEARCH for 1964

The report of the committee on Grants-in-Aid of Research as presented at the 64th Annual Convention of the Society of the Sigma Xi (pages 26A–28A March issue of the AMERICAN SCIENTIST) gave some indication of the magnitude of requests for assistance being placed upon the committee. At the March meeting of the Executive Committee, the funds available for awards in 1964 were increased from $80,000 to an all-time record high of $100,000, but even this effort will fall far short of meeting deserving requests for aid.

At the first meeting of the year which considered some 93 requests, 71 awards totaling approximately $41,000 were made. This amount represented 53% of that requested by those applications acted upon.

At the second meeting 130 requests were reviewed by the committee. Of these 96 were approved for awards totaling approximately $47,000, which amount was 45% of the total requested.

As this issue of the American Scientist goes to press, 146 applications are awaiting action by the committee at its third and final meeting of the year. These requests are for a total of $144,000 and there is but $12,000 remaining of the funds allocated for awards for Grants-in-Aid of Research for 1964.

Dr. Harlow Shapley, chairman of the Grants-in-Aid of Research Committee, has stated that it is his intention at the coming meeting to process the applications in order of the date of receipt. When the funds have been exhausted those whose application cannot be acted upon will be invited to withdraw their application or to request that they be considered at the first meeting for 1965.

The committee regrets very much its inability to satisfy at least in some part those applications which could qualify for aid if funds were at hand. It is hoped that the Society can continue to increase its aid for the encouragement of scientific research and that in 1965 funds to make this possible may be obtained.

Kodak reports on: **orderly rummaging . . .**
decent-sized color prints in 8 minutes

He is telling the man that a set of 11 microfilm magazines like the one in his hand and the one he has inserted in the top of the reader holds the 21,000 infrared absorption spectra that at least one publisher now sells in microfilm form. Other magazines (or rolls, aperture cards, film jackets, microfiches—all of which are compatible) will supplement this basic library with the man's own spectra. When matching "fingerprints" or when deducing the structure of a new compound by comparison with related ones, the larger the library the easier the task, provided the indexing system is good. When it yields a desired number, the man will insert the proper magazine and let the microfilm race until the code lines on the film, seen against the scale alongside the screen, find their mark. On the screen appears the desired spectrum. The piece of paper is a "hard" copy thereof, such as emerges from the slot in the base at a touch of that button he is pressing. Nobody gets a chance to ruin the integrity of the file.

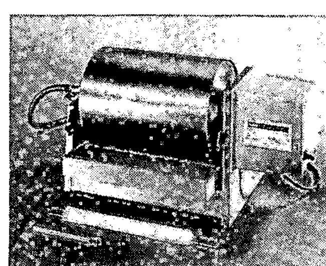
I-r for i-r's sake

Consider a man who holds down a job determining the identity of unknown substances. On his income tax return he has written "chemist." His family thinks of him holding a test tube up to the light and scowling at it. Actually, a far more typical working pose would show him rummaging through heaps of long strips of paper as he searches for an i-r absorption spectrum that some pinhead has misplaced. In moments of agonizing self-appraisal he knows that the way analytical chemistry is practiced today, a more orderly approach to information storage and retrieval may earn several times as many points as the ability to name from memory nine ways to test for urea.

Expertise on the i-r (information-retrieval, that is) problems of the modern analytical laboratory can hardly be expected to come from within. Recordak Corporation, 770 Broadway, New York City 10003, a subsidiary of Eastman Kodak Company, *can hardly wait* to send a representative who is well aware that every lab has to work differently, that some will have us do their microfilming for them, that some will want to lease the right kind of microfilmer and do it themselves, that some generate enough spectra to justify buying their own microfilmer, etc., etc.

A place for art and a place for hot water

Since just about any laboratory that ever undertakes photomicrography, metallography, or makes any other use of decent-sized color photographic prints would be embarrassed to have to explain undue delay in acquiring at least one of these devices, we had better explain what it does.



It converts a sheet of KODAK EKTACOLOR Professional Paper into an 11″ x 14″ color print in less than 8 minutes after leaving the enlarging easel. Using well under a dollar's worth of processing chemicals. And an operator who perhaps learned only the same morning of his interest in darkroom artistry. Who proceeds to deliver even and more reproducible results than can be expected from many high-rent square yards of workspace occupied by big trays. Because the drum spins at 200 ft/min. Whipping a homogeneous film of fresh solution over the print. Which is held stationary by the nylon net blanket. Pictured here rolled up as furnished. Along with the thermometer. For checking the hot water. That flows into the drum to keep it at 100°F. So that only 2½ minutes suffices for development. Followed by a tilt of the tray to dump the 4 ounces of developer. And 4-oz. chasers of each of the four other solutions. Punctuated by shots of plain water. With full roomlight permissible for 4½ of the 8 minutes. And that's about all there is to the perfect processing of an 11″ x 14″, 8″ x 10″, or pair of 4″ x 5″ color prints. When enjoying the freedom of choice in number and size of prints that the color-negative method leaves open to you.

Camera shops all across the country are asking less than $200 for this KODAK *Rapid Color Processor, Model 11. No, a digit has* not *been dropped through typographical error, but the price is subject to change without notice.*

This is another advertisement where Eastman Kodak Company probes at random for mutual interests and occasionally a little revenue from those whose work has something to do with science

THE SCIENTISTS' BOOKSHELF

By Hugh Taylor, the Associate Editors, and Guest Reviewers

SEE INDEX AT END OF THIS SECTION

The Collected Papers of Lord Rutherford of Nelson, Vol. II, Manchester; SIR JAMES CHADWICK, Science Director; 590 pages; $17.25; John Wiley & Sons, 1963.

On February 24, 1964, there appeared in "PHYSICAL REVIEW LETTERS" a report on the discovery of a new hyperon signed by no less than thirty-three physicists. Nothing could be in greater contrast with the papers written by Rutherford during his tenure of the Professorship of Physics at Manchester between 1907 and 1919. It would be a rewarding experience for every present-day student of physics to read these accounts of the experiments on which all of our later ones depend. In fact, the whole history of the application of quantum theory to atomic structure through spectroscopy, the quantum mechanics, and nuclear theory stem from Rutherford's work in this period.

He had decided earlier, it can be surmised, that the α-particle was a helium atom with two positive charges, but that belief had to be proved experimentally beyond the shadow of a doubt. So we find a considerable number of papers gradually making more and more certain of its nature, culminating in the experiment of Rutherford and Royds in which the α-particles were allowed to pass through very thin glass, to be collected and to exhibit the spectrum of helium.

The two most important results of Rutherford's researches were announced early in the period and very late, and both resulted from the scattering of α-particles by matter. The experiments on scattering of X-rays by heavy elements led to the conception of the nuclear atom, and the scattering by light elements revealed the existence of disintegration by α-particle bombardment. Both required the insight of a great physicist.

The physical means available to Rutherford and his students were very meagre, but they were supplemented by the most extraordinary ingenuity and finesse in the designing of the experiments. Throughout these papers one can see Rutherford's personal delight in his work. Quoting Professor Feather's INTRODUCTION to this book: "At the end of a long day he could say, simply, to a young colleague: 'Robinson, I'm sorry for those fellows who haven't got a laboratory to work in.'"—*A. G. Shenstone*

The Rat: A Study of Behaviour by S. A. BARNETT; 288 pages; $7.95; Aldine Publishing Co., 1963.

The new science of animal behavior has had a rather unique genesis in that it was created by the recombination of parts of several older disciplines. The two principal contributors have been (1) American psychologists rigorously schooled in the experimental techniques necessary for the study of learning and motivation in mammals, and (2) European ethologists whose painstaking naturalistic observations are generally concerned with species-specific behavior patterns in submammalian vertebrates and invertebrates. After a period of sometimes vitriolic disagreement, the two groups have reached a happy truce in which the efforts of both are considered complementary and necessary for a complete understanding of the behavior of animals. For this reason a book written by an ethologist about the psychologists' favorite subject, the

rat, will be eagerly welcomed by both. Unfortunately, *The Rat*, in general, does not meet expectations.

The early part of the book reviews the ecology, sociology, and fixed action patterns of the laboratory rat and its wild "cousin," *Rattus norvegicus*, with interesting comparisons to another wild species, *Rattus rattus*. The last half of the book is concerned with learning, motivation, and brain function. This section will perhaps prove valuable in introducing ethologists to the experimentation and theorizing of psychologists.

Throughout the book the author places stress on the precise definition of terms and new definitions are frequently given for old terms, ostensibly to increase their precision. If the former procedure had been done with consistency, and the latter had resulted in uniformly better definitions, a real contribution might have been made. Unfortunately, the book fails on both counts. For example, three definitions are given for "ethology." In the preface ethology is "the study of behaviour"; on page 4, "the scientific study of behaviour"; and in the glossary, "the scientific study of animal behaviour." The only obvious consistency here is in the British spelling. An example of a change in the meaning of an older term which results in loss of precision is the statement that "trial and error behaviour . . . is equivalent to, or includes, . . . 'operant conditioning' and 'instrumental conditioning' . . . " Operant behavior is most frequently "shaped" by the experimenter; quite a different process from an animal in a maze or a puzzle-box. Also, the glossary would have one accept the fact that "internal inhibition" is a synonym for "extinction" defined as a "decline in performance. . . . ." Such confusions do not serve to further the science of animal behavior.

On the positive side, the author has refrained from the use of such terms as "innate" or "instinctive." There may also be merit in dropping the terms "critical period" and "species-specific" in favor of "sensitive period" and "species-characteristic." The latter terms are more accurate and better describe the phenomena to which they refer.

In sum, although Professor Barnett has made many significant contributions to the study of animal behavior, *The Rat* cannot be considered one of them.—*Thomas E. McGill*

Methods in Immunology by DAN H. CAMPBELL, *et al.;* 268 pages; $8.75; W. A. Benjamin, Inc., 1963.

This lucidly written and thoughtfully detailed book satisfies a long-existing need for a laboratory manual of immunology which can be applied at both the undergraduate and graduate levels. Classical as well as the most recently developed methods in the science are comprehensively described in terms that should give no difficulty to students with some background in general biology and chemistry.

The book is divided into three sections: the first describes general biological and physical techniques which have found widespread application in immunological research; the second involves specific laboratory exercises which include applications of the techniques described earlier and introduce methods specific to immunology; the third concerns the preparation of special reagents, principally buffers, which are used in some of the exercises. Each of the laboratory exercises is preceded by a brief explanatory passage defining the principles involved. Of themselves, these are admittedly inadequate, as they should be in a laboratory manual, but pertinent references are given to guide students to more comprehensive treatments of the fundamental material. The selection of experiments touches on all important areas of contemporary immunology, including basic immunochemistry, hypersensitivity and immune tolerance.

Several defects, some of them typographical, were found, but they detract little from the usefulness of the book. On page 105, line 5, the sentence should read ". . . to remove antibodies *against antigens* that are common to all tissues." On page 135, a supernatant test is described for a precipitin curve which was established using increasing dilutions of antigen, while on page 140 the

quantitative precipitin determination is described using increasing concentrations of antigen, a more acceptable procedure. The switch in methodology introduces some unnecessary confusion. On page 205, in a discussion on determining antibody by combination with isotopically labeled antigen, a question is raised concerning errors introduced by coprecipitation of nonspecific serum proteins with the antigen-antibody complexes. Coprecipitation should not be a factor here because only radioactivity is being measured and the antigen is the only labeled protein. On page 231, line 4, the sentence should read "... white cell extracts...."

In conclusion, this book can be highly recommended as a source of laboratory exercises in immunology for undergraduate, graduate and medical students. The authors have admirably combined an extraordinary understanding of the minor technical pitfalls that frequently plague the novice with a clear presentation of some of the most advanced immunologic techniques.—*Joel W. Goodman*

The Developmental Anatomy of "Isoetes" by D. J. PAOLILLO, JR.; 130 pages; \$3.50 clothbound, \$2.50 paperbound; Illinois Biological Monographs, No. 31; University of Illinois Press, 1963.

This monographic treatment of the plant genus Isoetes will be welcomed by the comparative morphologist, the anatomist, and the morphogeneticist. It is a concise, clearly written, well illustrated, successful and timely effort to utilize the pattern of development as the approach to interpreting the organization of the mature plant, both as to organ formation and to internal anatomy. The genus Isoetes has proved puzzling to botanists. These "most interesting, provocative and enigmatic" vascular plants are clearly lycopsid in nature, yet completely different from others of the Lycopsida in their generally aquatic or amphibious habit; moreover, they lack known fossil progenitors. With vertically short, cormlike axes each bearing crowded quill-like leaves on its upper part and many stigmarian-like roots in a uniform but unique pattern on its lobed basal part, it is not surprising that, for more than one hundred years, attempts to understand growth patterns and growth habits have provided a considerable body of controversial interpretations. It is against this background that Dr. Paolillo has utilized a comparative approach, has employed newer techniques, and has produced a basic pattern of organization that seems to have resolved most of the fundamental problems discussed by earlier students of the group.

For the technical botanist who will welcome and will purchase this small book, it should be pointed out that the author has grouped the problems essentially as they have been envisioned by past workers. The three major headings are: the shoot and its meristems, both apical and lateral; the root-producing meristem; and the apical meristem of the root. It is in the new studies of these meristems and their patterns of behavior that Dr. Paolillo has found the "breaks through" that he sought.

The reviewer cannot refrain from expressing the hope that Dr. Paolillo will put his knowledge of Isoetes plants to work by making it the basis for experimental morphogenetic studies. Preliminary results suggest that the complete life history of at least one species of Isoetes can be carried out in sterile nutrient culture. Few such opportunities exist for broad morphogenetic studies on both the vegetative and reproductive level of any vascular plant. In addition, no other living lycopsid plant permits an examination of both primary and secondary activities for even the anomalous cambial activity of Isoetes can be exploited with a view to understanding why it is anomalous and what anomalous may mean as a morphogenetic phenomenon.—*Ralph H. Wetmore*

Diffusion in Semiconductors by B. I. BOLTAKS, translated from the Russian by J. I. CARASSO; edited by H. G. GOLDSMID; 378 pages; \$14; Academic Press, 1963.

After more than a decade of intensive investigation it is surprising to find

only this one monograph available. In spite of several excellent review articles and the extensive treatment of oxides in Hauffe's books, Boltaks has performed a useful service in bringing together the major items that relate to diffusion in the covalent semiconductors: mathematical treatment of diffusion equations, defect equilibria and mobilities, impurity solubilities and their effects on electron energy states, methods for measuring diffusion coefficients in solids, and reviews of experimental measurements and theoretical studies on various classes of semiconductors, including elemental semiconductors other than those of Group IV and oxides.

The first two chapters attempt to cover defects and impurities in crystals and diffusion theory in a very condensed fashion. As a result, incomplete or vague statements abound. On page 44, Figure 17, a potential energy surface with final states represented at much higher energies than initial states suggests some haste in assembling this material. The following chapter on theories of diffusion in covalent and ionic semiconductors is frequently suggestive, but often incomplete. The balance of the book is generally satisfactory and contains accounts of many interesting Russian studies. As in any monograph of this sort the critical reader is likely to disagree with individual statements and conclusions.

The subject index has approximately 160 entries compared with more than 100 entries in the table of contents. No name index is included. A name index and expansion of the subject index would have been valuable. The printing is light in places, especially in some of the equations and figures. Probably only those with a compelling interest or a financial stake in diffusion in semiconductors will choose to read this book. Such reading should be supplemented by comparison with the original literature whenever critical judgments must be formed.—*C. E. Birchenall*

Genetics by ROBERT C. KING; 347 pages; $7.50; Oxford University Press, 1962.

The first thing that strikes one in looking at *Genetics* is the fact that it is superbly illustrated and that the printing is excellent. The benefits do not stop there because the book actually turns out to be very well written and a true representative of modern genetics.

The problem of the complexity of modern genetics that is currently in a stage of explosive expansion comes to a head when you have to write a general text in the field. The author, in this case, has presented a very clear outline of general genetics and made a successful attempt at bringing in the newer aspects of molecular biology. This book has achieved a real balance in these two aspects of the subject. The author has not fallen into the usual trap of presenting the newer data in great detail before its ultimate significance is weighed with equally interesting but not timely older genetic material.

This book would be excellent for an intermediate or beginning text at the college level. If it is somewhat short in certain areas, it is also clear in its organizational pattern.

In addition to being superbly illustrated, there are a number of very useful tables in the appendices to the book in terms of the chronology of genetics and useful bits of information on the materials used in genetics and the general reference materials.

The text *Genetics* ought to be around quite a while as a real contender for use in the standard college genetics course.—*Bruce Eberhart*

Material Behavior in Mammals, edited by HARRIET L. RHEINGOLD; 349 pages; $8.75; John Wiley & Sons, 1963.

If scientists in more hard-nosed disciplines than mine (psychology) can bring themselves to part with the $8.75 required to purchase this book they will be richly rewarded. It will teach them that behavior *can* be subjected to scientific inquiry and that not all psychologists are either junior grade head-shrinkers, laboratory students of part processes, or windy speculators. Further, they will find that both anthropology and zoology have a lively in-

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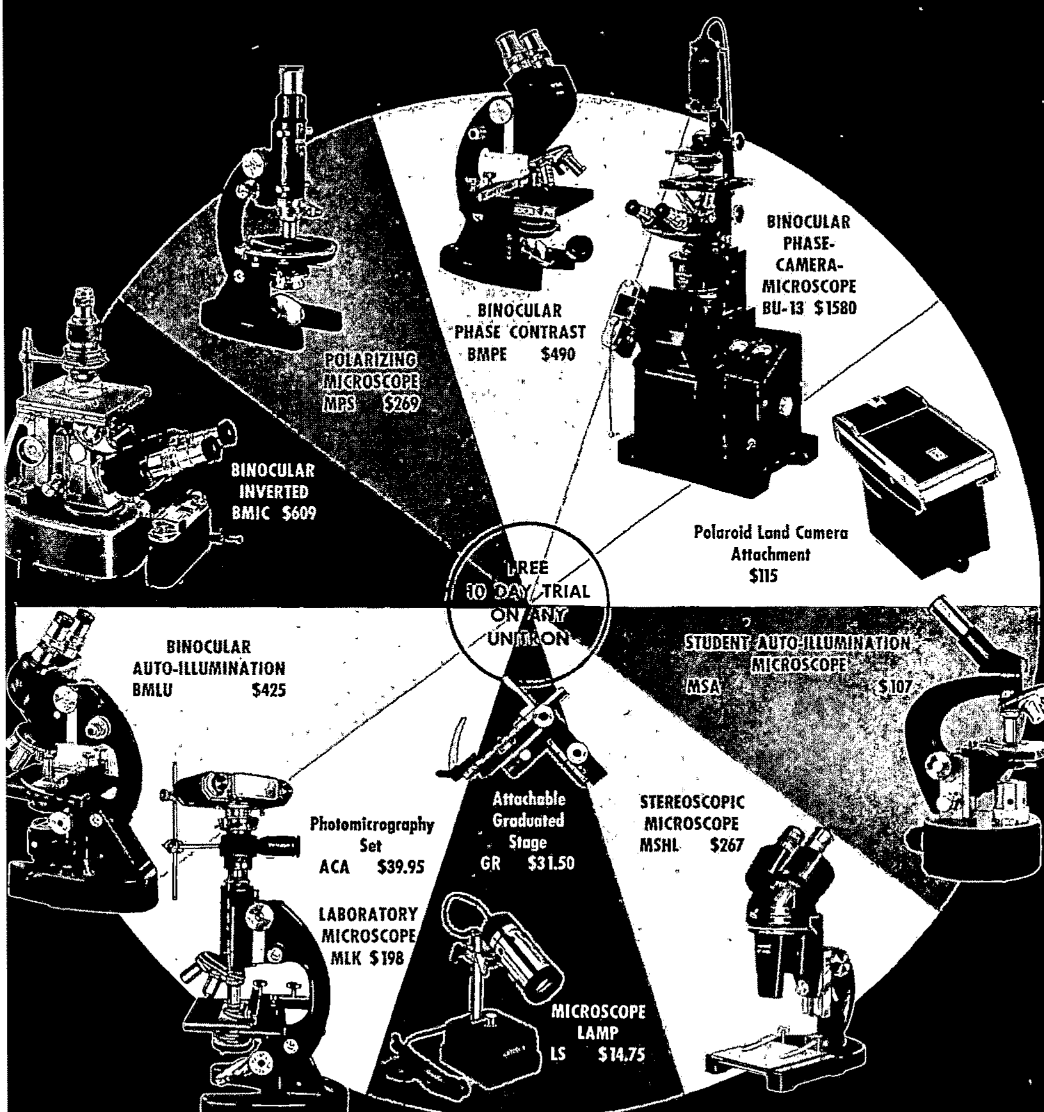

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terest in the living beast and his important daily behavior. Those scientists who are mothers will be charmed. Those who are only married to mothers may well increase their wives' respect for the scientific calling.

Twenty authors have produced ten chapters covering twelve species of infrahuman mammals: rat, peromyscus, rabbit, cat, dog, sheep and goat, moose and elk, rhesus monkey, langur monkey and baboon. To the human nonspecialist the eighty pages on primates will probably be most interesting, but my daughter studied the rat and cat with an enthusiasm that her physiology instructor would applaud (though her French teacher would hardly believe!). Yet this book is no popular digest for high school students—original empirical work ranging from the laboratory to field studies is reported in detail.

Dr. Rheingold's introduction (she is also the author of the paper on the dog) points out that "the term 'maternal behavior' has proved somewhat troublesome." One of the troubles is that among Irven DeVore's baboons—a generally terrestrial species, thus subject to the attack of predators—the adult males turn out to have enormous protective, and even caretaking, importance. Indeed, they exercise supervisory functions over the juveniles that are perhaps similar to those exercised by older sisters in many human groups. But in Phyllis Jay's much more arboreal langurs, the sexual dimorphism is much less and so is the protective and caretaking importance of the adult males. The functional and evolutionary significance of these differences is clear.

One last word. All the authors are acutely aware that "maternal" behavior in these complex forms is *social* behavior determined in great part by the young (and his peers or littermates) as well as by the adult. Read it.—*John P. McKee*

Zone Electrophoresis in Blocks and Columns by H. BLOEMENDAL; 219 pages; $7; American Elsevier Publishing Co., 1963.

Since Tiselius described the principles of electrophoretic separation in 1937, a large number of techniques have been advanced for isolation of specific charged substances and analysis of ionic mixtures based on electrophoresis. In recent years nearly all such methods, both preparative and analytical, have utilized the principles of zone electrophoresis where an inert material is used to inhibit convection of ions migrating in the applied electric field thereby allowing more complete and efficient separations. In this monograph, Dr. Bloemendal attempts to present a practical guide to zone electrophoresis (restricted to blocks and columns and excluding paper electrophoresis). He is eminently successful in this attempt.

Electrophoresis in blocks and columns using granular or gel stabilizing media, continuous electrophoresis, and column electrophoresis using density gradients as anti-convection devices are the subjects covered. Under each topic, the necessary apparatus, a large variety of different stabilizing media and their preparation, application of sample mixture, factors affecting the electrophoresis such as pH and ionic strength of the supporting electrolyte, temperature, applied voltage, current, etc., location of substances of interest in the stabilizing medium, and the elution and estimation of such substances are all discussed in detail. Some of the applications and important biochemical findings resulting from the use of the various types of zone electrophoresis are briefly reviewed. The presentation of such biochemical findings without more extensive discussion of their significance in interpreting other results is a minor fault in the book. The many excellent illustrations are particularly helpful in understanding the practical sections.

More extensive discussion of such practical problems as electrosmosis (briefly treated), constant current *versus* constant voltage power, further application of zone electrophoresis to separation of small molecules, reproducibility and other analytical aspects of zone electrophoresis, and similar problems would have been desirable. However, these omissions detract only slightly from the value of the work. This book should stimulate "Techni-

cal improvements, which aim at quantitative work under well-defined and reproducible conditions . . ." (Arne Tiselius). This book is highly recommended to those now using or who contemplate the use of zone electrophoresis. —*David B. Straus*

Applied Mechanics Reviews WADEX, Volume 15, 1963, datamated by E. A. Ripperger, *et al.;* 576 pages; $7.50; The American Society of Mechanical Engineers, New York 17, 1963.

Workers in several areas of science and engineering are familiar with the monthly publication *Applied Mechanics Reviews*. Each year this publication gives a brief critical review of almost 8000 technical papers that have appeared in journals throughout the world. These reviews are listed under subject headings with cross indexing, and in an author index. A *Word and Author Index* (*WADEX*) for reviews published during the year 1962 has been prepared by means of computer techniques.

WADEX consists of a single alphabetical index, with listings both under descriptive words from the title and under the author's name. Each listing gives the full title, author's name, and reference number to the *Applied Mechanics Reviews*. The volume covering 1962 fills nearly 600 pages with double columns, representing in the order of 40,000 entries. Each review must appear in *WADEX* at an average of five different places. The volume represents an effort to prepare a useful and complete index in a consistent form, with the intellectual effort needed in the preparation reduced to a minimum. It is an interesting experiment, and its editors welcome constructive comments. —*W. J. Cunningham*

Recent Developments in Network Theory, edited by S. R. Deards; 250 pages; $12.50; The Macmillan Co., Pergamon, 1963.

This latest monograph in a series in aeronautics and astronautics comprises the Proceedings of The Symposium on Network Theory held at the College of Aeronautics, Cranfield, England, in September 1961. Since few such symposia have been held in England, this volume holds a particular interest for the network theorist.

Fourteen papers are included in this volume, in three major classifications: Linear Passive and Active Network Theory, and Non-Linear Network Theory. Each classification is subdivided into sections on analysis and synthesis. Since it is difficult within the space of this review to give even the briefest comments on each paper, I will indicate the areas of prominent coverage, and cite several of the papers.

Nine of the fourteen essays consider several aspects of passive, linear network theory. Four of the nine consider n-terminal networks, presenting matrix analysis techniques, two realization theorems, and a transformerless synthesis procedure to realize a grounded N-port with a prescribed impedance matrix.[1] Three of the nine consider classical synthesis problems. A review of the present state of the art by K. M. Adams strives to connect the fundamental results of various circuit synthesis procedures which have been reported to date. This survey has a discussion appended to it in which Dr. Brayshaw poses the query, "on the principle that there is usually a basic simplicity in fundamental results, may it not be that complicated restrictions met within the particular examples cited here are a warning that this approach is wrong?" In a rebuttal, Dr. Adams admits that classical network theory is often inadequate and quite clearly incomplete. He expresses the hope that topological methods now being researched will yield stronger and more fundamental procedures; nonetheless, Dr. Adams implores the reader not to disavow the sometimes aesthetically dissatisfactory but significant classical methods. Extensions and generalizations of both Brune's work and the Bott-Duffin procedures are considered in the next two papers by Dr. Talbot and Dr. Brayshaw. J. W. Head discusses "Metri-

[1] Published previously as Monograph 531E of the Institution of Electrical Engineers, July 1962.

cal Mathematics and Operational Calculus." In a paper rather removed from the rest, Dr. Duijvestiyn considers, within the area of graph theory, the problem of dissecting a rectangle into squares.

Two papers of more than usual interest make up the section on Active Network Synthesis: Dr. Sarag's Approach to Active RC Synthesis, and Keen's nonreciprocal synthesis procedure. In the latter, a generalized nonreciprocal element, the unitor, is introduced. This reviewer particularly enjoyed both of these papers.

The three remaining papers discuss flow graph analysis and nonlinear circuit theory. Perhaps, in reaching an overall impression of the impact of this volume, this reviewer is forced to agree with Brayshaw, who admits his frustration at the complicated and sometimes seemingly unrelated synthesis procedures which are the building blocks of contemporary network theory.—*Ralph W. Wyndrum, Jr.*

Group Theory and Its Application to Physical Problems by MORTON HAMERMESH; 509 pages; $15; Addison-Wesley, 1962, and *Group Theory, the Application to Quantum Mechanics* by PAUL H. E. MEIJER AND EDMOND BAUER; 288 pages; $9.75; John Wiley & Sons, 1962.

Although the central book on the application of mathematics to the symmetry of physical problems—E. P. Wigner's *Gruppentheorie*—was published in 1931, it has only been in the last few years that use of group theory has enjoyed widespread popularity. Current interest is largely attributable to the extensive efforts being made by physicists and chemists to understand quantitatively complex nuclei, atoms, molecules, and crystals. This interest has given rise to half a dozen recent books with as many different orientations and styles. The book by Hamermesh stands at one end of this spectrum. Its presentation is complete, clear and accessible to physicists and a few chemists, but in spite of its title, its style and contents are primarily that of applied mathematics. It will appeal to that readership which desires a more complete mathematical development than offered by the more physically oriented books such as M. Tinkham (McGraw-Hill, 1964) or V. Heine (Pergamon Press, 1960). Discussions of the symmetric group, the Young tableaux, the structure of Lie algebras, and irreducible tensors are noteworthy in this regard. In addition to the classification of symmetry groups and treatment of representation theory, Hamermesh gives applications to such topics as the seniority number in atomic and nuclear spectroscopy and the little groups of solid state theory.

The first five chapters of the book by Meijer and Bauer (vector spaces, principles of quantum mechanics, formal development of group theory, representation theory, and rotations in three dimensions) are a translation from a 1933 French monograph. These and the remaining three chapters (Representations of tensor operators, space groups, and finite groups with some applications) suffer pedagogically from being too brief to provide a genuine introduction to group theory but not advanced or complete enough to form a reference of specialized text. Nevertheless there are useful short sections on the Wigner-Eckart theorem, Racah coefficients, little groups, Kramer's theorem and the Jahn-Teller effect.—*Leland C. Allen*

Genetics & Modern Biology by G. W. BEADLE; 73 pages; $2; Memoirs of the American Philosophical Society, Vol. 57, 1962 Jayne Lectures, 1963.

The advances in our understanding of the nature of heredity during this century are among the most striking in the whole field of science, and perhaps the most spectacular of these is the interpretation in biochemical terms that has been developed especially in the past ten years. We have come a long way from the time when Bateson could say, in 1916:

"... it is inconceivable that particles of chromatin or of any other substance, however complex, can possess those powers which must be assigned in our factors [genes.] ... the supposition that

particles of chromatin, indistinguishable from each other and almost homogeneous under any known test, can by their material nature confer all the properties of life surpasses the range of even the most convinced materialism."

In this short account of 73 pages Beadle has given an admirable summary of the history and present status of the subject. The summary gives a vivid impression of the excitement of scientific research, and of what kinds of people carry it out, as well as bringing the current views up-to-date.

There are three chapters: "Mendel to Watson and Crick," "In molecular terms," and "Evolution and the nature of man." Of these, the third is perhaps least obviously described by its title, since it is concerned largely with the evolutionary implications of the nucleic acid codes and their functions in protein synthesis. There is a brief bibliography, and no index.

The book is to be strongly recommended as an accurate and readable report that should be intelligible to the general reader.—*A. H. Sturtevant*

The Physiology of Mosquitoes by ALAN N. CLEMENTS; 393 pages; $12.50; The Macmillan Co., Pergamon, 1963.

Although Havelock Ellis may have overstated the case when he epitomized mosquitoes as "all of Nature gathered up at one point, in her loveliness, and her skill, and her deadliness, and her sex," the life processes of these insects have, over the years, attracted a great deal of research interest. Dr. Clements has compiled much of the pertinent literature on the subject in his "Physiology of Mosquitoes." The title was interpreted in its broadest sense to include genetics, embryology, behavior, etc., but excludes comparative reference to other insect groups. The bibliography contains nearly 800 citations, of which more than half were written within the last decade. Tabulation, by year of entry, reveals that their numbers appear to be steadily increasing and, if the trend continues, the bibliography of a book of similar scope written at the end of the next decade would again be doubled. This book is well indexed and the bibliography, which is complete through 1961, includes a few references published in 1962. Unpublished data that were derived from theses, annual reports, program abstracts, and personal communications are discussed in some detail. This practice has the unfortunate effect of perpetuating error.

Dr. Clements has minimized his editorial function in the writing of this book. There are no general introductory or summarizing statements and matters of controversy are frequently treated by omitting mention of other points of view. As a result, the subject would appear to the uninitiated to be relatively stable and well explored. This, of course, is not the case.

The scope of this book is so great that a uniform quality of discussion might not be expected throughout. Examination of the section dealing with autogeny, an area in which Dr. Clements has made contributions, reveals significant disparities. He neglects the paper of Twohy and Rozeboom (1957), whose results are somewhat at variance with his own, and misstates the results of Spielman (1957). While similar disparities may be found in other sections of the book, many topics are accurately presented. Especially valuable are the detailed discussions of recent German reports concerning genetics, embryology and host seeking.

While this book must be read with constant reference to the original literature, it is a useful addition to a library of Medical Entomology, serving as a convenient guide to the recent literature of a rapidly expanding and complex subject.—*Andrew Spielman*

Invertebrate Zoology by ROBERT D. BARNES; 632 pages; $10; W. B. Saunders Co., 1963.

The welcome volume fulfills the longstanding need for an American text of invertebrate zoology at the upperclass and graduate level. The difficult task of presenting a balanced treatment of the taxonomic, anatomical, physiological, embryological, and eco-

logical aspects of the invertebrates in phylogenetic sequence has been performed in a scholarly, modern, interesting manner.

The use of biological types is abandoned; instead each major taxon is treated from a general standpoint, facing the student with a broad spectrum of facts and exceptions and an opportunity to achieve a mature and realistic appreciation of the nature and diversity of the invertebrate phyla. Frequent mention is made of species commonly utilized as laboratory specimens, facilitating the use of the book in connection with laboratory studies. Phylogenetic information provides continuity between chapters and an overall unity. A valuable contribution is the discussion of opposing theories and unresolved questions.

Twenty chapters cover the protozoans, origin of the metazoa, sponges, coelentrates, ctenophores, flatworms and origin of the bilateria, nemerteans, spiral cleavage and embryology of turbellarians and nemerteans, pseudocoelomates, annelids, mollusks, introduction to arthropods, trilobites, chelicerates, crustaceans, myriapods, annelidan and arthropodan allies, lophophorate coelemates, echinoderms, and lesser deuterostomes. Sporozoa, Trematoda, Cestoda, and Insecta were omitted as these classes are covered adequately in parasitology and entomology texts, leaving space for additional material on other invertebrates.

The book is readable, has a pleasing format, is relatively free of typographical errors, and is plagued by very few statements which the critical reader might find objectionable. Systematic resumés of major taxa, and useful, often lengthy, bibliographies after each chapter will be valued by the good student. Unfortunately the illustrations, although liberally used, are considerably below the general standards of the rest of the book, and constitute its weakest feature.

This volume will probably find wide usage in American colleges and universities. It is strongly recommended.—*Melbourne R. Carriker*

Advances in Organic Chemistry: Methods & Results, *Vol. IV*, edited by R. A. Raphael, *et al.;* 361 pages; $14.50; John Wiley & Sons, Interscience, 1963.

The volume under review continues to provide the same high standard of reference material in some of the most useful synthetic tools of organic chemistry, as the preceding volumes. Three chapters dealing with the synthetic chemistry of enamines, synthetic methods in the carotenoid and Vitamin A fields and the coupling of acetylenic compounds to polyenynes of the acyclic and cyclic types form the contents of this volume.

Dr. Szmuszkovicz, one of the coll aborators of Professor Stork in the development of enamine chemistry, has naturally written a very impressive discussion dealing with the syntheses, structures and the many different types of acylations and alkylations possible with the enamines. This chapter makes stimulating reading, as the author looks at the enamine molecule from many diverse angles in a multitude of structural framework.

Drs. Isler and Schudel provide one of the most well-documented reviews on the wide variety of synthetic approaches possible to long chain polyenes. Extensive discussions give insight to the new types of reagents and reactions, more recently discovered, in building up a carotenoid molecule. Copious references are given to procedures like the Nef reaction, the Wittig type condensation, the enol ether condensation the Aldol condensation, reductive dimerization and the Robinson-Mannich type condensation. In spite of the fact that part of this discussion has been covered in different earlier reviews by other authors, this chapter certainly offers a fresh approach and makes for a comprehensive summary of the topic in a unified manner.

With the expanding interest in naturally occurring polyacetylenes and the aromatic character of monocyclic polyenes, the coupling of acetylenes has assumed major synthetic importance in organic chemistry in recent years

largely due to the efforts of the schools led by Professors Jones, Sondheimer and Bohlmann. Professor Eglinton and Dr. McRae have provided a wealth of information in this direction, amply supplemented by tabular surveys and extensive bibliography.

The editors are to be congratulated on the choice of the topics for this volume as well as for giving an opportunity to the authors to bring their references up-to-date to work published in 1962.—*B. S. Thyagarajan*

Hydrodynamic Superposability by R. Ballabh; 45 pages; $3.75 paper; Taplinger Publishing Co. for Asia Publishing House, Bombay, 1963.

Two constant-density viscous flows with velocity fields q_1 and q_2 are termed superposable if there exists another such flow with velocity field $q_1 + q_2$. In the present very short monograph is found a study and review of superposable and self-superposable flows, based primarily on earlier papers of the author. The listed price is unreasonably high.—*W. D. Hayes*

The Evolution & Eradication of Infectious Diseases by Aidan Cockburn; 255 pages; $7.50; The Johns Hopkins Press, 1963.

The reader of this inevitably controversial volume should first get the author's background of experience and contacts and his general philosophy by a careful perusal of the Preface and Acknowledgements and the initial chapter on The Role of Speculation in Research. He should then turn to the chapter covering his own particular interest and knowledge; "the specialist in any one of the fifty odd disciplines to which reference is made is likely to find errors and omissions within his special knowledge" which will prepare him to anticipate similar errors and omissions in other disciplines. The reader is now ready to enjoy and profit from Cockburn's "attempt to give a birdseye view of the relationships (past, present, and future) between host, parasite, and environment with particular reference to human infectious diseases."

The author's view of such relationships is given in chapters on: Evolutionary Background; Paleoepidemiology; the Evolution of Infectious Diseases; the Species Concept of Microbiology; the Basic Principles of Eradication; the Treponematoses; Cholera; Smallpox, Poliomyelitis, Tuberculosis; and Eradication and the Population Explosion.

The ten chapters have a total of 241 wide-ranging bibliographic references; unfortunately many of these references are too indefinite to be helpful, as that to Strode's 700-page work on yellow fever.

The volume is beautifully edited and adequately illustrated with tables, maps, and photographs.

The reviewer has found what he considers "errors and omissions within his special knowledge" and divergences from his own opinions; these were to be expected with an author whose sweep is so wide.

The Evolution and Eradication of Infectious Diseases is recommended reading for workers in the biological sciences; especially should it be in the hands of public health workers unfamiliar with the concept of eradication in the prevention of communicable diseases.—*Fred L. Soper*

The Physics of Engineering Solids by T. S. Hutchison & D. C. Baird; 368 pages; $8; John Wiley & Sons, 1963.

This book has many commendable pages. But, to publish a text-book on fundamental material already treated at various levels in many books requires going beyond what is already available: it must be significantly more up-to-date (is a brief mention of lasers sufficient distinction?) or its presentation must be superior in clarity and organization. This is not the case here. The authors stress the incompleteness of the treatment of almost every topic so frequently that one soon gets a feeling of despair—in spite of encouragements such as: "The full significance of this becomes clear later . . ." In the first

few chapters there are fine illustrations of crystal structures but weak support from the text. The praise-deserving sections are Chapter 4 on diffusion, part of Chapter 5 on elastic and plastic properties and especially Chapter 6 on wave mechanics which is beautifully suited to the intended user (the engineering student). Then the text rapidly soars to a high level of sophistication. A surprise recipe for making copper oxide rectifiers sneaks in after some 200 pages of pure physics. The chapter on semiconductors is rather weak and there are incorrect statements about excitons. The frequent cross-references to previous and to subsequent entries are annoying. The lazy student will enjoy the simple problems and the given answers. Let us terminate by blaming the editors for a layout of figures and tables which makes the text choppy.—*J. I. Pankove*

Nuclear Physics, An Introduction by W. E. BURCHAM; 739 pages; $12; McGraw-Hill Book Co., 1963.

Burcham has attempted to provide an introduction to almost the whole field of nuclear physics, excluding only practical applications and high energy physics, and has done suprisingly well at such an ambitious task. Intended for senior undergraduate or beginning graduate students it is written from an experimental point of view.

The book is divided into four major sections, beginning with the historical development of nuclear physics from the days of Becquerel and Rutherford to the present time. The second section deals with the experimental techniques used in the field and here the book should be especially valuable to beginning graduate students since Burcham has managed to include almost all the present-day techniques. The second half of the book takes up the theoretical aspects of nuclear physics. The third section considers nuclear models and static properties of nuclei. It gives a good introduction to the various nuclear models and their ranges of applicability. The fourth section treats nuclear reactions and scatterings as well as the dynamic properties of the nucleus. In order to cover such a wide range of material, the treatment of most subjects is necessarily brief, but nevertheless quite readable.

The lack of any problems reduces the usefulness of this book as an undergraduate text but this lack is not important for a graduate text or reference book. It would be a fine review of the subject for the more advanced student and can be highly recommended to those entering the field of nuclear physics and also to those in related fields who are interested in nuclear physics.—*C. A. Kelsey*

Fast Neutron Physics, Part II: Experiments & Theory, edited by J. B. MARION & J. L. FOWLER; 1308 pages; $45; John Wiley & Sons, Interscience, 1963.

The second and final part of this comprehensive work deals primarily with experimental results and the theories used in their interpretation. It is a collection of 20 articles by 33 authors, each an expert in his field. To cover such a wide field with contributions from so many authors could lead to a series of disconnected chapters, but Marion and Fowler have done a fine job of maintaining continuity. They have arranged the articles to form a smooth and continuous treatment which starts with the more easily understood features of neutron interactions. The first three chapters are devoted to the average effects of neutron interactions and to the optical model which is based on these average properties. A chapter on direct interactions provides a transition between the gross nuclear properties and resonance phenomena. More complicated subjects such as polarization and inelastic scattering follow the treatment of resonances. The second half of the book is concerned with nuclear reactions involving particles other than neutrons. A chapter on the fundamental *n-p* interaction concludes the book.

Each of the chapters is self-contained and can be studied independently. Most of the chapters on theory start by explaning the basis for the model and

then develop some of the simpler results of the theory. Finally, the more advanced and involved aspects of the theory are treated. The experimental chapters attempt to give a complete description of experimental progress to date. Unfortunately, Part II was delayed in publication for over two years and the editors comment on the revisions necessary because of this delay. Each chapter provides a comprehensive survey of the pertinent literature up to sometime between 1957 and 1961. Only careful reading of the chapters will determine the most recent reference on a particular subject. The chapters on experimental results appear to suffer more from the delay in publication than do those on theory.

Part II can be recommended to both physicists and nuclear engineers who are interested in reviewing the fundamental theories of neutron physics as well as in keeping up with the more recent advances in the field. Unfortunately the price will prevent many from adding it to their personal library.—*C. A. Kelsey*

Basic Astronomical Data, edited by K. AA. STRAND; 495 pages; $12.50; University of Chicago Press, 1963.

This is the third volume of nine in the series *Stars and Steller Systems* under the general editorship of Kuiper and Middlehurst. In this volume, twenty-eight astronomers have contributed nearly as many authoritative articles reviewing the fundamental observational data relating to the stars, such as motions, distances, luminosities, temperatures, and masses. In each chapter the observed parameters are carefully defined and, in some cases, the theoretical implications are discussed. The errors in the measurements and the uncertainties in their interpretations are well treated by most authors. Where space permits, definitive tables of values are included. All the articles give references to the sources and the best compilations of the data.

Inertial reference frames and the proper motions of the stars, so essential to our knowledge of galactic structure, are described in five articles by Clemence, Scott, Vasilevski, Dieckvoss, and Luyten. Keenan and Strömgren have contributed chapters on spectral clarification, while Johnson, Becker, Sharpless, Hall, and Serkowski have written on multicolor photometry, interstellar reddening, and polarization. Harris has compiled the best estimates for the temperatures and bolometric corrections of the stars, necessary data for the comparison of theories of stellar evolution with the observations. Other authors have described visual and eclipsing binary systems and the information they provide on stellar masses and radii. Blaaw has an important article on the determination of absolute magnitudes in which he discusses all the methods used and thoroughly analyzes the errors involved. Kraft and the Gaposchkins have added chapters on the luminosities of cepheids and other variable stars.

It is regretable that this most useful collection has taken so long to appear. The editor admits receiving most of the material in 1960, but several manuscripts were delayed until 1962, postponing the publication and requiring revisions to the early articles. However, major changes in the fundamental data now occur relatively slowly and this volume will remain an essential reference for such information for many years.—*Donald C. Morton*

The Sun & the Amateur Astronomer by W. M. BAXTER; 167 pages; $5.95; W. W. Norton & Co., 1963.

The aim of this book, as stated by the author, is to draw the attention of amateur astronomers to the sun. This book fills a gap in astronomical literature between technical books describing the work of professional observatories and books on general astronomy which briefly mention the sun as one of many celestial objects. Drawing upon his own experiences, Mr. Baxter communicates the great satisfaction gained from serious, systematic observations. He points out the excitement of watching the ever-changing panorama of solar activity and, most important, that

these observations can be made with simple, inexpensive equipment.

In the first half of the book the author describes the sun in simple language and with numerous, original analogies. The simplicity of the explanations is such that the elementary school amateur can easily grasp the picture presented, and yet the style of writing is pleasant and interesting to the well-informed. It is a pleasure to see that the accuracy of the facts is not impaired by the translation from technical to layman's terms. The thoroughness of the description is also commendable. Some important facts and concepts are explained that even some "advanced" books overlook, and these are explained well without recourse to terminology which the novice might not understand. This combination of simplicity, interesting literary style, thoroughness, and complete accuracy makes this book a valuable addition to astronomical literature.

The second half of the book is concerned with the tools and methods of observation. It is very appealing to the beginner to see the emphasis on extremely simple and inexpensive observing aids. The description of instruments is confined to the very smallest. This is in contrast to descriptions for observing other heavenly bodies, which invariably frustrate the beginner and owner of small telescopes by including photographs or glowing descriptions of the superior views through large telescopes. Mr. Baxter ends his discussion of instruments with descriptions of his own modest equipment.

The work of amateur astronomers would be more highly valued if all would adopt observing methods as exact and thorough as those described here by Mr. Baxter. The book carefully outlines how to make accurate measurements of sunspot areas and positions and how to keep precise records of all observations. The final pages of the book are devoted to solar photography, ending with a collection of splendid sunspot photographs taken by the author with a 4-inch refractor.

Mr. W. M. Baxter has been a member of the British Astronomical Association since 1932 and is now the secretary of the Association.

In summary, this reviewer is impressed by a book which accomplishes its purpose so well.—*Patrick S. McIntosh*

Human Nature in Politics by J. C. Davies; 403 pages; $7.50; John Wiley & Sons, 1963.

Davies sets as his task the specification of the ways in which the fundamental conditions of human life affect man's political behavior and political institutions. He begins by selecting (from Maslow) a set of basic human needs and analyzing their significance for such diverse phenomena as the relative politicization of nations, the stability of hierarchical status structures, and the motives of political leaders. He discusses personal tensions and perceptual biases as forces in determining the opinions and actions of elites, and their contributions to political stability and change.

Groups are analyzed in terms of a distinction between proximal and distal groups, with the assertion that "Routine, everyday proximal group experiences, mainly by way of an individual's family, are the most potent social influences" (p. 189). The effects of families, political parties, religions, ethnic groups and status groups upon individual political behavior are assessed on the basis of this assumption. Davies concludes with provocative analyses of motives for identification with leaders, of the social origins of leaders, and of the conditions for major political change.

This work is especially significant from two points of view. First, Davies has made a substantial attempt to work from empirically-based generalizations about human behavior. The gaps in our knowledge about crucial aspects of political behavior are thus made abundantly clear. The empirical base is strongest in his discussions of physiological needs and of the influence of groups. It is weakest in his discussions of cognitive and social needs, and of the origins and effectiveness of leaders. Happily, Professor Davies makes use of a literate and broad knowledge of political history to provide appropriate

examples when empirical evidence is weak or missing. Second, he has offered an original and thought-provoking set of hypotheses about the constraints which "human nature" places upon political institutions. His work grapples intelligently with the central issues of political behavior, and can be recommended to any social scientist or educated layman.—*David O. Sears*

Passages From the "Idea Books" of Clark L. Hull. Perceptual & Motor Skills Monograph Supplement 9-V15, 1962 by R. B. AMMONS *et al.*, 82 pages; $3; Southern University Press, 1962.

This monograph adds three articles to the Psychology of the Scientist Series initiated by Ammons and Ammons in 1962. Ammons furnishes background information on the monograph and recalls Hull's position in psychology. He describes the "passages" as "an intensely personal and private document, a kind of emotional and intellectual diary."

Ruth Hays masterfully details the setting in which Hull's ideas were born during his last 22 years. Hull's close associates will note how she has caught the true flavor of life in the Hull Unit. The descriptions sympathetically reflect the depth and greatness of Hull. Yet nowhere does Ruth Hays indicate the key role which she herself played in the Hull Unit during those productive years.

The major portion of the monograph consists of excerpts or "passages" selected by Ruth Hays from Hull's "Idea Books." These 76 pages only hint at the contents of the entire set of 75 books in which Hull recorded his personal thoughts and creative ideas over a period from 1902 to 1952. However, the excerpts are so skillfully selected that one is able to trace much of Hull's life from high school to within several months of his death. The major phases of Hull's productive life are clearly outlined, including aptitude testing, the correlation machine, hypnosis, conditioning and the formulation of his systematic approach to behavior theory. His contagious enthusiasm is very evident, but one also sees plainly the tremendous labor, the constant striving and the disappointments which were so much a part of Hull. Throughout the passages one can feel Hull's strong drive for success as well as his absolute devotion to and confidence in the scientific method. Occasional comments reveal Hull's greatest fear, that senility or death would keep him from accomplishing all that he hoped.

No doubt many readers will find these passages so interesting that they will look hopefully toward the time when more of the contents of the "Idea Books" may become available. Certainly this monograph should be on the list of required readings for every graduate student entering the field of experimental psychology.—*C. T. Perin, Jr.*

The Design of Electric Circuits in the Behavioral Sciences by TOM CORNSWEET; 329 pages; $8.95, John Wiley & Sons, 1963.

The writer or teacher of medical electronics is faced with the necessity for making some prior judgments of the sophistication of his audience and the amount of information which he can reasonably impart to them relative to their needs. The author of this book has assumed that his reader has no prior knowledge and has produced a kind of preprimer of medical electronics which furnishes both a basic vocabulary and a primitive grammar. As such it is a useful addition to the scanty literature in the field.

The danger in this approach is that the reader will derive a kind of pseudosophistication, the ability to provide "cookbook" instrumentation and a totally insufficient awareness of the precision, flexibility and enormously greater utility of more reasoned approaches.

The initial third of the book is devoted to a basic and lucid treatment of electrical phenomena and laws. Two chapters on switches and related circuits stress mechanical components, are slanted towards operant conditioning circuits and ignore the digital logic ap-

proach and the variety of useful modules available from commercial sources. The function of counting is discussed in relay terms while counting as a basic operation in timing is not presented. A short chapter on shock is generally adequate but there is insufficient mention of the necessary precautions to be employed with human subjects.

The final chapters of the book on amplifiers, feedback systems and sensors, which represent the materials of greatest interest to the behavioral scientist, are inadequately developed. Transistors are briefly mentioned but there is no discussion of the numerous uses for other members of the semiconductor family.

This somewhat overpriced book provides good basic information for the electronically naive but would be of questionable value to anyone other than the beginner.—*Clinton C. Brown*

Trends in the Mental Health Services, A Symposium, edited by H. FREEMAN & J. FARNDALE; 341 pages; $10; The Macmillan Company, Pergamon, 1963.

Recent years have seen a large increase in our optimism about treatment of the mentally ill and a comparable increase in the types and flexibility of services provided. The principal emphasis has been on movement away from the traditional, isolated mental hospital pattern and toward the community. The present symposium volume reflects the intense British concern with these developments and their resulting debates on how best to direct them.

The reader should not be put off by this being a symposium volume. Some papers are original, others reprinted; the whole hangs together better, without the usual symposium diffuseness, and is more attractively printed and bound, than its average American counterpart. There is also a good deal more humor, sometimes playful, sometimes mordant, than we allow ourselves. This is a great relief.

Many of the principal issues that involve psychiatric hospitals, general hospitals, community services for the mentally ill, and day hospitals, are ably discussed and from diverse but related points of view. The general atmosphere is one of lively experimentation. The articles' considerable specific material concerning the background of British developments will be especially useful in making comparisons with our American situation. What the American reader will chiefly miss is any detailed discussions of the place of psychotherapy in planning; nor are we given much in the way of dynamic interpretations of treatment or administrative events. Both hiatuses reflect significant differences between American and British psychiatry.

The volume reminds us, however, that there are two facts equally true in both countries. The British, like ourselves, suffer from a grievous lack of manpower through much of their mental health work and from difficulties in financing that are only beginning to improve. The symposium convincingly illustrates how far ahead of training and available funds are intentions, plans and enthusiasm. This is an account of valiant struggles to do much with very little.—*Leston & Nancy Havens*

Nuclear Research Emulsions, Vol. I: *Techniques & Theory* by W. H. BARKAS; 518 pages; $18; Academic Press, 1963.

The use of nuclear emulsions for detection of charged particles has evolved in the last decade into a sophisticated technique capable of providing quantitative information of high precision. In the hands of the particle and nuclear physicist this technique has had a very successful history. However, so far, the nuclear emulsion tool has not been widely adopted in other fields in the physical and biological sciences although it holds considerable promise of being valuable in a variety of applications. Perhaps this lack of familiarity with the technique may be attributed in part to the difficulty in acquiring the information necessary to master the art from the existing literature. This hurdle has now been overcome with the publication of this excellent book on nuclear research emulsions written by one of

the outstanding authorities in the field.

In this volume Dr. Barkas presents a complete and comprehensive treatment of the technique and theory of nuclear track emulsions. Tasks for which emulsions are useful are discussed along with the advantages and disadvantages of the technique. The various types of emulsions commercially available, their physical properties and relative merits, are all discussed. There are chapters presenting in detail the theory of the latent image, processing procedures, and measuring equipment. About half of the volume is devoted to a thorough treatment of the measuring methods used to identify and determine the velocity of the particle whose path through the plate has been rendered observable by this photographic process. Each of these topics is first treated in an elementary way before being further developed in more sophisticated analysis. There is a considerable amount of new, unpublished material in the book as well as compilations of material widely scattered in the literature. Virtually everything that one would need to know to master this technique, as well as gain an understanding of the theory of the process, is given in this book. A second volume in preparation will treat in more detail the study of elementary particle physics with emulsions.

The particle or nuclear physicist already using the emulsion technique will find this book invaluable, and probably has already obtained a copy. Rather, I would like to draw the book to the attention of researchers in the biosciences, earth sciences, and space sciences who are involved in problems of detecting charged particles. For those who wish to familiarize themselves with this technique and explore the possibilities of using it in their work, this book is highly recommended. For those researchers who would like to adopt this technique in their laboratory, this book is a necessity.—*Oliver E. Overseth*

The Process of Evolution by P. R. EHRLICH & R. W. HOLM; 347 pages; $8.95; McGraw-Hill Book Co., 1963. (1st publication in McGraw-Hill's Population Biology Series).

This book is an attempt to present the *process* of evolution rather than its end products. It is free from the belabored proofs that evolution has happened, which take up so much of other works in this field. In the middle sections of the book there is an excellent, up-to-date, although simple, presentation of population ecology and genetics with most of the important recent work in genetic assimilation, disruptive selection, meiotic drive, the founder effect, and patterns of spatial distributions. All the familiar cases, such as industrial melanism, polymorphism in Cepaea and Drosophila, and Darwin's finches are described. The prevailing views in the field are accepted for the most part, although with some careful qualification.

Controversial questions are generally by-passed, but there is a militant criticism of the biological species concept. It is clear that many organisms, including asexual clones, polyploid series, and composite organisms such as lichens, are not organized into Mayrian species. However, the authors insist that, even for sexual diploids, the determination of good species is extremely difficult, making the concept useless. This does not follow. If the biological species is a unit of evolutionary process then it must be recognized as an analytic entity even if the boundaries are necessarily fuzzy. Nobody will deny the existence of China and India even if their border is at least ambiguous.

The interesting sections of the book are embedded in additional material for completeness, treating subjects further from the authors' own areas of special interest by perfunctory summaries from the major texts. The discussion of the origin of life is on the level of introductory biology courses and the paleontological discussion is straight Simpson, down to the ubiquitous graph on the survival of genera of land carnivores and pelecypods. In the human evolution section, Coon's notion of polyphyletic origins of *Homo sapiens* from *H. erectus* is uncritically accepted.

The weakest part of the book is the discussion of social evolution. Here we find an oft-repeated sample of "last chapter" ideas many of which were introduced to anthropology by biologists, accepted in the border areas where ignorance provides license for uncritical speculation, and requoted by biologists from the anthropology texts. These cover the origins of agriculture and religion, intergroup selection, selection against intelligence, a proposed role of love, sex, and kinship in the choice of political systems, and dubious parallels between biological and cultural evolution (e.g., Hitler as a social mutant). The objection raised here is not to speculation, but to non-thoughtful speculation of a kind that would be impossible in the biologists' discussion of Lamarckism or special creation. Thus, what could have been an excellent little book has been expanded into a less satisfactory full-length text.—*R. Levins*

Progress in Nucleic Acid Research, Vol. I of An International Series, edited by J. N. DAVIDSON & W. E. COHN; 424 pages; $13; Academic Press. 1963.

This book, the first in a series, provides workers in the field of nucleic acids with opportunities for more personal interpretation, discussion and speculation than is normally possible in review articles. Succeeding volumes will appear as sufficient material is gathered, and not according to arbitrarily fixed dates of publication.

The subjects covered by the first volume may be visualized from the titles of the articles: "Primer" in DNA Polymerase Reactions; The Biosynthesis of Ribonucleic Acid in Animal Systems; The Role of DNA in RNA Synthesis; Polynucleotide Phosphorylase; Messenger Ribonucleic Acid; The Recent Excitement in the Coding Problem; Some Thoughts on the Double-Stranded Model of Deoxyribonucleic Acid; Denaturation and Re-

naturation of Deoxyribonucleic Acid; Some Problems Concerning the Macromolecular Structure of Ribonucleic Acids; The structure of DNA as Determined by X-ray Scattering Techniques; Molecular Mechanisms of Radiation Effects. The author or authors of each subject are leaders in their particular areas, and present a bird's-eye view of the present status of our knowledge. Out of the eleven articles, at least nine fulfill the editors' intention of seeking "expression of points of view that are perhaps controversial and certainly individualistic," and offer stimulating reading.

Among the numerous books appearing like "bamboo shoots springing out after a spring shower" on similar subjects, this volume occupies an unique position in that the information is thorough, reliable, and presented with abundant imagination and critical comments. The reviewer recommends the book highly and feels it will be extremely useful both for scientists at work in similar and related fields, and for students who seek sound, up-to-date knowledge on these subjects.—*Noboru Sueoka*

Introduction to Modern Biochemistry by P. KARLSON; (English translation of 3rd German edition, 1962, by C. H. DOERING); 433 pages; $10; Academic Press. 1963.

The author has attempted to provide a concise introductory text which surveys the principal facts and ideas in biochemistry. This goal is essentially accomplished in the 24 chapters devoted to a description of biochemical events at levels extending from the basic metabolic pathways to the special biochemical functions of certain organs. However, the objective of conciseness is attained at the price of excluding or devoting all too brief attention to several of the current exciting areas of research in biochemistry. For example, the reviewer could not find any discussion of the enzyme control processes as feed-back inhibition or enzyme repression which are important in the control of microbial metabolism and may also play a significant role in metabolism in higher organisms. In addition, several of the significant contributions to the study of gene action made by advances in microbial genetics are just briefly mentioned or presented in a manner which does not convey to the reader the excitement of achievements in this area in recent years. To the book's credit, however, the author has succeeded in treating the less spectacular subjects as the basic chemical properties of amino acids or simple sugars in a more dynamic manner. This more interesting presentation is partly due to the excellent illustrations which are found throughout the book. One of the illustrations involves a large fold-out chart summarizing the principal reaction pathways and their interactions. No attempt has been made to treat extensively the biochemical aspects of clinical problems and the methodology in biochemical research is discussed only in so far as it describes several of the newer important methods.

In general, the author has done a commendable job in sorting out the enormous amount of detail associated with a description of the many areas of biochemistry and has provided a text which presents the principal facts and concepts in these areas in a concise and interesting manner. For this reason, the book is highly recommended to the beginning student of natural science as an introductory text to biochemistry.—*Donald R. Helinski*

Comparative Endocrinology, Vol. I: Glandular Hormones, edited by U. S. von EULER and H. HELLER; 543 pages; $20; Academic Press, 1963.

Volume One is vertebrate endocrinology discussed from the comparative viewpoint. The book begins with a discussion of hypothalamic control of the pituitary. This topic, of prime importance to all endocrinology today, is the most slighted of any topic discussed in the volume, only 18 pages, with no attempt to be comparative. There then follows a series of chapters on the usual categories of vertebrate hormones—neurohypophyseal, melanophorestimu-

lating, female and male gonadal hormones, adrenocortical, thyroid, parathyroid, and chromaffin cell hormones, insulin and glucagon. The book concludes with a chapter devoted to the comparative biochemistry of adenohypophyseal hormones and a chapter on the physiology of the adenohypophyseal hormones.

Each section is written by a prominent worker in that respective area. The authors have done a splendid job of the comparative approach with clear discussions of the present state of knowledge or complete lack of it for each of the vertebrate classes. However, in some instances the authors have been rather selective both in subject matter discussed about a certain hormone and in the references employed. One sometimes got the impression that he was reading a summary of summaries, which is not always extremely enlightening. It appears probable the authors may have been forced into this by a lack of space to review a literature which is colossal in scope.

Despite the above objections to certain presentations, this rat endocrinologist, while reading this volume, learned many amazing facts about the other vertebrates and was stimulated to think about some comparative approaches to his subject. The comparative material assembled in this volume will make it of value for many years to come.—*Robert D. Lisk*

Advances in Enzymic Hydrolysis of Cellulose & Related Materials, edited by ELWYN T. REESE; 290 pages; $12; The Macmillan Co., Pergamon, 1963.

This book represents an outstanding attempt to bring the current knowledge of biological cellulose decomposition into a cohesive and workable framework. Thirteen papers by leading people in this field are drawn together in an interesting and useful form. Each paper is independent of the others, but its position in the book has been grouped according to the main problems of cellulose decomposition. These papers successfully demonstrate the state of the field up to this point. The problem of accessibility of enzymes to the cellulose in plant walls, the interference of substances other than cellulose with the reaction, and the opposing problem of multiplicity of cellulose enzymes are

the main themes that underlie this organization. A number of theoretical projections are presented as to the nature of cellulose breakdown by enzymes. This makes the book equally useful to a theoretical chemist as to a technologist.

This review will probably have some historical significance as a crystallization of thought prior to a major breakthrough in cellulose metabolism. It points out the many problems of cellulose metabolism and in some cases suggests their solution On one hand, there is a complex series of substrates and, on the other, an exceedingly complex series of enzymes. Another major problem is to obtain better methods of getting the enzymes directly from the cell before they are complexed and complicated by the action of proteinases and complexing polysaccharides in the external culture filtrate.

Nowhere in the book is it suggested that the modern techniques of genetics might be applied to the solution of the multiplicity of cellulases. Certainly this is going to be necessary before one can say how many polypeptides are involved in each enzyme and how many structural genes are involved for the production of each cellulase.

This book contains an extremely useful bibliography of the work between 1950 and 1961 assembled by F. E. Cole and Kendall King. The bibliography alone is worth the price of the book.—*Bruce Eberhart*

Newer Methods of Preparative Organic Chemistry, Vol. II, edited by W. FOERST, translated by F. K. KIRCHNER; 417 pages; $14.50; Academic Press, 1963.

This volume is a selection of fourteen reviews on preparative organic and biochemistry which originally appeared in *Angewandte Chemie.* The use of the following compounds in organic syntheses is discussed: acetoacetaldehyde, 1,3-cyclohexanediones for preparing long-chain carboxylic acids, ethyl 2-cyclopentanonecarboxylate, ketene, and phenylsodium. The preparation of peptides and ureas using reactive amides or imides, the chemical synthesis of intermediates of carbohydrate metabolism, and the preparative and analytical importance of phosphines are reviewed. The reduction of carbonyl compounds with complex hydrides, oxidations with noble metal catalysts and lead tetraacetate, the alkylation of aromatic amines and phenols with alkenes, and amidomethylation are included.

The authors are well-known authorities in their fields and in most cases have covered the newer aspects of their subjects with clarity. The style and emphasis vary, ranging from purely theoretical discussions to the chapter on the continuous preparation of phenylsodium which is concerned with laboratory details for the preparation and use of this material.

Chapters on subjects which have been reviewed elsewhere have been limited to emphasizing recent trends.

The discussion on amidomethylation is one of the most interesting and shows that the reactions of some amides with formaldehyde to give N-hydroxymethylamides can be utilized to prepare primary amines by condensations in acidic solutions. This is a versatile reaction which has only begun to be investigated. Another outstanding review is on the preparation of long-chain carboxylic acids from 1,3-cyclohexanediones. This chapter contains a number of tables and experimental examples.

This book should have wide appeal.—*Millard Maienthal*

Advances in Neuroendocrinology, edited by A. V. NALBANDOV; 525 pages; $7.50; University of Illinois Press, 1963.

This volume which is the result of a symposium held in Miami in December 1961 represents a milestone in the endocrinological literature of this country. It is the first American compilation of the field of neuroendocrinology and to achieve this end over forty scientists from Europe as well as America have contributed to the present volume.

The emphasis throughout has been on the role played by the central nervous system in the regulation of endocrine function. Starting with a survey of analyser-integrator systems that might be involved in endocrine phe-

nomena, the discussion moves on to an analysis of hypophyseal vascularity followed by a section on the origin of the neurohypophyseal hormones. This is followed by a review of the role of the central nervous system in the regulation of the various tropic hormones of the pituitary. The physiology of the pituitary gland following stalk section or transplantation is then reviewed. A survey of the recent literature on the chemistry of neuroendocrine mediators of central nervous system origin is also included.

Interactions between the brain, uterus and ovary are discussed and the importance of environmental factors is touched upon, especially the role of light in the neuroendocrine system. The ability of certain drugs to block neuroendocrine phenomena is discussed. Finally, the state of our knowledge on central nervous system regulation of endocrine function in the human is reviewed.

The presentations are well written and the authors have made fairly exhaustive reviews of their topics. In one case 700 references are listed. Thus, the bibliographic value of the volume in itself is enormous. A good attempt has been made to present all sides of the story and point out where controversy exists.

The only minor criticism that might be made is that the work included in this volume has already been published, so that the volume is in essence an excellent review of neuroendocrinology until 1960. It might have been made even more stimulating by the authors including their current thoughts and attacks on the subject.

For a reasonable understanding of the diversity of methods applied and the results achieved in the field of neuroendocrinology, this volume is a must.—*Robert D. Lisk*

Polymer Single Crystals by PHILLIP H. GEIL; 560 pages; $16; John Wiley & Sons, Interscience, 1963.

The discovery of chain folded polymer crystals in 1956–57 independently by Till, Keller and Fischer proved to be

more than just an exciting experimental event within the small world of polymer morphologists. Experimental and theoretical workers from several related fields immediately joined efforts to disclose the fundamental structural secret underlying the beautiful electron micrographs of single crystals that were being published continuously in the scientific journals. This was the beginning of a revolution in thinking which firmly placed the study of crystalline polymers in the domain of solid state physics. The complete record of this revolution is available for the first time in a thorough treatise by Geil.

After a brief review of the appropriate instrumental techniques, Geil proceeds to discuss the whole spectrum of habits (single crystals, hedrites, spherulites) exhibited by a great variety of synthetic polymers when they are crystallized both from solution and from the melt. The important operations of annealing and orientation are covered copiously in separate chapters. The two rival theories of chain folding ("kinetic" and "equilibrium" theories) are exposed lucidly and their relative merits are assessed, leaving one with the impression that the kinetic theory appears to be carrying the day in the face of accumulating experimental evidence. Complete references, including most of the very recent ones, are given throughout the book (Geil's inclusion of the titles of articles to which reference is made is very useful and is commonly practiced only by authors and publishers in the Biological Sciences). The quality of reproduction of the numerous photographs is high.

Aside from some rather casual references (e.g., p. 303), the vast field of crystallization of natural polymers is omitted. To this reviewer, the omission is symbolic of a reversal in attitude: Prior to the 1950's, concepts of molecular organization in synthetic polymers were largely derived from earlier work on naturally occurring semicrystalline fibrous materials (keratin, cellulose, silk). It would seem to me now that recent discoveries made on synthetic polymers, particularly since the unequivocal establishment of chain folding, should have significant bearing on the understanding of the crystalline structure of proteins and related macromolecules. In this respect, Geil's authoritative book could be read profitably by molecular biologists and biophysicists.—*J. B. Yannas*

Plant Metabolism by G. A. STRAFFORD; 152 pages; $2.75; Harvard University Press, 1963.

What the Harvard University Press regards as "an authoritative and up-to-date account of plant metabolism" is a very disappointing book. Although the publication date of this book is June 1963 there is no significant account of plant metabolism since 1959. One gathers from the introduction that the book was written for advanced high school students in England primarily because the University Entrance and Scholarship examiners frequently protested that they received ill-digested and poorly understood accounts of modern theories. In spite of such a statement the author has put together a very uninteresting account of certain aspects of plant biochemistry and physiology which is sadly out-of-date and strewn with misconceptions and errors.

In all, there are six chapters: "General Biochemical Principles," "Photosynthesis," "Nitrogen Metabolism," Mineral Nutrition," "Translocation" and "Respiration." The last five of these lack information on the role of the chloroplast in photosynthesis, the role of DNA in protein synthesis, the nature of the cell membrane and its role in mineral nutrition and translocation, mitochondria and localization of enzymes associated with the respiratory process. The author frequently refers to isotopes of carbon and nitrogen without outlining the methods by which they are measured in studies on metabolism. In fact little is said of the role of modern techniques in furthering our knowledge of plant metabolism.

Finally, the cover of the book portrays an organic chemical structure containing a trivalent H atom. It is surprising that the Editorial Board of

the Harvard University Press accepted such a text for publication and one can only wonder who was responsible for the cover.—*Raymond F. Jones*

Restoration of Function After Brain Injury by A. R. LURIA; 277 pages; $10; The Macmillan Co., Pergamon, 1963.

This book is translated from the Russian. It was originally published in 1948. It is based on material collected during and after the Second World War, by what is now the Institute of Neurology, Academy of Medical Sciences of the U.S.S.R. It is devoted to the analysis of the mechanism of disturbance of complex systemic functions by localized injuries of various parts of the brain and to the study of the different types of rehabilitation training which must be given to encourage the compensation of the resulting disability. Three types of restoration of brain function are distinguished. First by de-inhibition of temporarily depressed functions, secondly by the use of substitution of the opposite hemisphere and thirdly by the radical reorganization of functional systems. Other things being equal, the success of restoration of function depends on the severity of the brain wound, the volume of brain tissue affected, and the presence of complications on one hand, and the age and state of the brain before injury on the other. The preservation of a steadfast and intensive motivation, stabilizing the patient's inclination to work on the compensation of his defect is a further and important factor for the successful restoration of disturbed function.

There are many theoretical considerations that are difficult to accept. The successful treatment of some patients with prostigmine is almost too good to believe, and has not been borne out by similar trials in this country. However, it is an excellent attempt to explain on a physiological basis, how function is or can be restored following brain injuries, and it would be well worthwhile for all working in this field to read.—*Preston Robb*

Introduction to Geological Microbiology by S. I. KUZNETSOV, *et al.*, translated from the Russian by PAUL T. BRONEER, edited by CARL H. OPPENHEIMER; 252 pages; $8.95; McGraw-Hill Book Co., 1963.

This is an account of the occurrence, growth requirements, and biochemical activities of bacteria and allied microorganisms in sedimentary deposits, ground waters, and elsewhere in the crust of the earth. Discussed are various ways in which such microorganisms affect conditions and the diagenesis of organic compounds as well as many minerals in geological formations. Most thoroughly treated is the role of bacteria in the formation and modification of sulfur deposits, including sulfide ores. Shorter chapters are devoted to the geomicrobiology of iron ores, ground waters, and fossil fuels. Economic applications and implications of the geochemical activities of bacteria are outlined in a 20-page chapter. Only four pages are devoted to stating the problems and outlining the methods of geomicrobiological research.

A vast amount of widely scattered information on an important subject is brought together in this volume, which will be of interest to many microbiologists, geologists, chemists, engineers, and others. It is an excellent English translation of the original Russian version published in 1962. The addition of an index and several explanatory notes by the editor enhances the usefulness of the book. The extensive bibliography, complete with titles, is worth the price of the book. Of the 392 references, 280 are by Soviet authors. The senior author, Kuznetsov, has been publishing papers on the part played by bacteria in the modification of soils, sediments, ground waters, mineral ores, and fossil fuels for more than 30 years. His own extensive observations coupled with those of his colleagues and the published findings of many other workers have been summarized in a succinct and scholarly manner. Less chauvianistic than many Soviet scientists, the authors are as critical of the work of their compatriots as of western scientists.—*Claude E. ZoBell*

The Evolution of the Metazoa by J. HADŽI; 499 pages; $14; The Macmillan Co., Pergamon, 1963.

It is a welcome event when an hypothesis, grown fat and complacent through widespread acceptance and thus having gained the stature of a *ruling theory*, is boldly challenged. Hadži's book thus challenges the more general views on the family tree of animal phyla. He and Steinböck have cast the gauntlet before; the book at hand is an extensive and mature, though partisan, discussion of the problem.

Basic to Hadži's views is a complete break with the Haeckelian approach which has continued to permeate most evolutionary schemes: To Hadzi, cleavage patterns and larvae are simply ways of developing an adult organism, and are just as subject to divergent, parallel, and convergent evolution as are the adults. Thus there is no room in his scheme for a gastraea theory, a planula ancestor, and similar Precambrian phantoms.

The sponges (Parazoa) are seen as an independent metazoan branch, developed from colonial flagellates. The Eumetazoa, on the other hand, are derived from ciliate-like ancestors. Ciliates are held to be not "unicellular" but hermaphroditic "polykaryons," showing the beginnings of a three-layered body which characterizes the Eumetazoa. Evolution is supposed to have proceeded through the plasmodial, acoelous Turbellaria to the "higher" members of this group, and thence onward to the remainder of the "phylum Ameria," and the remaining three phyla-Polymeria, Oligomeria, and Chordonia.

The Radiata (Coelenterata (= Cnidaria) and Ctenophora) are demolished as a group: Cnidaria and Ctenophora are basically bilateral and have developed a somewhat radial symmetry in adaptation to their sessile mode of life (a view supported by the fossil record). Both are independently derived from the Turbellaria. The cnidarian polyp is held to be the basic form, not the medusa, and a good case is made for considering the Anthozoa as the more

"primitive," the Hydrozoa as the most divergently evolved cnidarians. Cnidarians and Ctenophores are evolutionary blind alleys.

In the phylogeny of the higher invertebrates, Hadži shows little patience with the traditional importance attributed to the "coelome," which is held to be a basket term for various independently evolved cavities, to the derivation of the mesoderm, and to the division into Protostomata and Deuterostomata.

The strongest argument which can be advanced against Hadži's scheme is the occurrence of cell cleavage—and of a particularly elaborate kind—in the turbellaria embryo, even in those turbellarians which then become plasmodial (insofar as they have been studied). The significance of this is, at present, largely a matter of faith.

This is the summation of the work and thought of one of the Masters of Invertebrate Zoology. In taking issue with all of the other schemes which have been seriously proposed, it high-lights many aspects of zoology. It tends to stray from its topics, is repetitive, and would be more effective at half its length, but it is elegantly written and translated.—*A. G. Fischer*

Optimum Design of Digital Control Systems by JULIUS T. TOU (Vol. X of *Mathematics in Science and Engineering;* RICHARD BELLMAN, Series Editor); 186 pages; $7; Academic Press, 1963.

The book presents the method of dynamic programming as a tool for the design of optimal digital control systems. Because of the particular problem considered, dynamic programming has certain advantages over other techniques such as the Maximum Principle and calculus of variations. The characteristics which strongly suggest the use of this method are the discrete time nature of the systems and the use of quantized control signals. It is a well-written book, suitable for students and specialists in the field of digital system design. There are problems (but no answers) included for students and some

discussion of computational aspects of the problems.

Much of the work reported in the book represents research results by Professor Tou and his students. The material was available in their reports and technical papers at an earlier date but the book gives a unified presentation of their results. Specifically, the topics covered include general principles of optimal design, optimal control for multivariable processes, optimal control for processes with inaccessible state variables, optimum estimation of state variables, optimum control for random-parameter processes, design for optimum quantization and time responses for optimum quantized systems.

The printing of the book is satisfactory despite several typographical errors. One drawback of the book as a text for a course is inadequate reference to the literature concerning specific mathematical results. Properly interpreted by a teacher, this should not provide significant difficulty. The scope of the book makes it suitable for part of a graduate course in modern control theory if supplemented by appropriate reference material.—*Stephen J. Kahne*

The Moon, Meteorites & Comets, Vol. IV of *The Solar System*, edited by B. M. Middlehurst & G. P. Kuiper; 810 pages; $15; The University of Chicago Press, 1963.

The present volume is the fourth of a series entitled *The Solar System* and the second edited by the team of Kuiper and Middlehurst.

There are twenty-two chapters in this long book. Five chapters deal with the moon, six contain discussions of distributions of meteorites on earth and meteoritic craters, while three chapters are concerned with the physics, chemistry, ages, and evolutions of meteorites. Chapters 15 though 20 discuss comets, their orbits, structure, dynamics, and origin. The last two chapters are on meteors and interrelations between meteors, meteorites, and comets.

The mark of a good book of this type is the skill with which it is put together. Of the four current volumes of this series, this one was undoubtedly the most difficult to design, for it covers three major, independent fields of study, yet the information gained in each field often has some direct bearing on the other disciplines.

There isn't a weak paper in the group. Each chapter is by a recognized authority, often by a pioneer in the field. A high level of scholarship is regularly demonstrated, coupled with an ability to write so that the nonspecialist can understand.

Solar-system astronomy is moving so rapidly that there are often hundreds of references on each obscure point. It is becoming evident that collections such as these are of prime importance to the specialist in relating his work to that in associated fields. It is still more evident that the dilettante or professional in other lines can in no other way gain as broad a picture of solar-system astronomy.

The Moon, Meteorites and Comets is highly recommended to all interested in our solar system. The four volumes of this set already published and the fifth, which will be soon issued, will make a valuable reference set for many years to come.—*Ralph B. Baldwin*

Steroid Reactions: An Outline for Organic Chemists, edited by C. Djerassi; 657 pages; $9.75; Holden-Day, Inc., 1963.

The chemistry of steroids, unlike that of the alkaloids, offers an unusual field for exploration of specificity of many reagents and diverse transformations because of the common rigid ring skeleton found in the entire class. Such a rigid framework offers the possibility of carrying out selective changes at multiple sites uninfluenced by each other. The volume under review is a compilation of the common steroid reactions organized to emphasize their selectivity, mildness of conditions or superiority in yields.

Fourteen different chapters cover the following reactions: protection of carbonyl and hydroxyl groups, selective oxidations and selective reductions, introduction of fluorine into the ring

system, preparation of alpha halo ketones and their dehydrohalogenation, introduction of double bonds, the Birch reduction, other metal-ammonia reductions, photolytic preparative methods, selective aromatizations, conversion of 20-ketosteroids into 17-ketosteroids, ring contraction of steroids, insertion of heteroatoms into the ring, preparation of alpha hydroxy and alpha acetoxy ketones and formation of epoxides and episulfides.

The subject matter is covered almost exclusively by structural formulae except for brief introductory discussions at the beginning of each chapter. A welcome feature in this compilation is the indication of reactions which fail, with reasons for such failure.

As Professor Djerassi has aptly remarked in his introduction to the book, this book would be highly valuable when used in conjunction with Fieser and Fieser's *Steroids.*—*B. S. Thyagarajan*

Symposium on Marine Microbiology, edited by CARL H. OPPENHEIMER; 769 pages; $22.50; Charles C Thomas, 1963.

The marine environment is the most extensive one available to living things on this planet. In the oceans, primary production, as well as degradative activity, is carried out almost entirely by microscopic organisms. Thus, marine ecology starts and ends as marine microbiology.

In April 1961, a first *Symposium on Marine Microbiology* was held in conjunction with the sixty-first annual meeting of the American Society of Microbiologists. More than sixty of the world's workers in the field of marine microbiology contributed, and the resulting published volume gives a good sampling of activity and trends in research at that time.

The editing and organizing done after the meeting delayed publication until two years after the symposium was held. The result appears to justify the time spent, as it is relatively free from the disjointedness often found in such volumes. The scope and comprehensiveness of the book recommend its use as a text or as supplementary course

reading. Workers in other fields will find it a convenient introduction to recent research in this area.

The sixty-six "chapters," i.e., papers, which comprise the volume are divided into an introduction and six sub-headings titled: "The Producers and Their Relation to the Chemical and Biological Environment," "Geomicrobiological Activities of Marine Microorganisms," "Ecology of Algae, Protozoa, Fungi and Viruses," "Heterotrophy in Marine Microbiology," "Distribution and Function of Marine Bacteria," and "Marine Bacteriology and the Problem of Mineralization."

The literature citations from the various papers are relisted alphabetically in an appendix as a combined bibliography of over eight hundred entries which could serve as a good introduction to the literature of the field.—*William S. Maddux*

Principles of Paleoecology (An Introduction to the Study of How & Where Animals & Plants Lived in the Past) by D. V. Ager; 371 pages; $10.75; McGraw-Hill Book Co., 1963.

Dr. Ager has produced, within the narrow limits of some 300 or more pages of text, an admirable survey of the state-of-the-art in paleoecology. The book begins with a brief, general consideration of the subject. It then turns to autecologic interpretation and deals successively with interpretation from living relatives, from morphology, from orientation, from fossil associations, from evidences of activity, from sedimentary associations, from lateral variation and geographic distribution of single taxons, and from changes in habitat. Ager then discusses synecologic problems and various modes of interpretation including comparison with living assemblages, geological criteria, density and diversity, interspecific interrelationships, and the temporal and spatial distribution and changes in assemblages. He concludes with an analysis of several paleoecological syntheses.

The text is complemented by two useful appendices, a glossary and a field questionnaire, and by a comprehensive bibliography. Ager emphasizes, throughout, the analysis of examples and draws from these a statement of interpretative methods that is far more comprehensive than any available heretofore. His expression of examples and methods is clear and effective.

To this reader the deficiencies of the book, both as a scientific review and as a possible text, are in the explicit avoidance of principles in spite of the title. Although this lack of grand design reflects truthfully the present status of the discipline, I regret that he did not see fit to express himself on principles, however tentative such statements would be. In the absence of such an attempt, the book tends to break down into a series of distinct and isolated discussions of interpretative approaches, and the concluding chapter on paleoecologic synthesis fails to attain the desired integration. Even accepting Ager's rejection of a theoretical approach, a more complete analysis of fewer examples might be a more effective pedagogic approach.

These criticisms, however, are obviously an expression of a personal bias, and Ager has achieved an admirable work within the scope of his defined goals.—*James R. Beerbower*

Pharmacology of Carotid Body Chemoreceptors by S. V. Anichkov & M. L. Belen'kii, translated by R. Crawford; 225 pages; $8.50; Macmillan Co., Pergamon, 1963.

This is an excellent book in which the pharmacology of the carotid body chemoreceptors is treated extensively. A good deal of the information available in this book comes from work done in Professor Anichkov's laboratory over a period of many years. Bibliographical references to Western literature are ample; there is, however, emphasis in the quotation of Soviet authors. This emphasis should be welcome to Western readers since a number of the periodicals quoted have been available only in the Russian language until very recently. This fact may explain why, in recent reviews on the pharmacology of carotid

body chemoreceptors, earlier work by Soviet authors has been neglected.

The book is divided into nine chapters starting with one devoted to the effects of poisons which produce tissue hypoxia; it is followed by two chapters; one pertains to the action of substances affecting cholinergic and adrenergic processes and the other to the effects of other pharmacological agents. These chapters are followed by others which deal with some theories on the mechanism of chemosensory excitation where some biochemical mechanisms, possibly responsible for excitation in these receptors, are well treated. This is a valiant effort, since the biochemical mechanisms which may elicit activity in these receptors are practically unknown. The book ends with one chapter devoted to some special concepts of the authors related to the initiation of the chemosensory discharges followed by a chapter dedicated to reflex effects induced by stimulation of these receptors.

In general, the book is a balanced one, where the authors' concepts about excitation mechanisms are evolved from the experimental data and reached with restraint. Excessive speculation is clearly avoided. As far as this reviewer is concerned, this book is probably the most extensive and well-documented treatise on the pharmacology of these receptors. It is not a mere review where work of different authors is frequently presented in sequential and, at times, uncritical fashion. Presentation of work done by others, together with the authors' own experiments, makes the end result an enjoyable one, since the writers know the field well indeed. In short, this book ought to be consulted by all persons interested in the subject.—*C. Eyzaguirre*

Structure & Ultrastructure of Microorganisms (An Introduction to a Comparative Substructural Anatomy of Cellular Organization) by E. M. BRIEGER; 327 pages; $10; Academic Press, 1963.

The title of this book promises a broad view in a growing field and one that taxes the powers of assimilation and ex-

position of any one person. Dr. Brieger's noble enthusiasm does not, alas, protect his book from superficiality, obsolescence and redundancy. Throughout, there is a wasteful overlapping with several review articles on bacterial organization without reference or a crystallization of views; the more puzzling because the same publisher commissioned several of them. A consideration of comparative cytology and the "concept" of a bacterium without reference to several not so new essays (especially those of Stainer and van Niel) seems to us less than complete. The comparative aspect, which justifies "microorganisms" in the title, is a textual patchwork largely concerning functional elements and with viruses in an appendix by Horne and Waterson (useful but limited in scope to symmetries and subunits in negative staining). It is curious to find a discussion of chloroplasts and preplastids in plant cells but no consideration of the reasonably well-known organization of photosynthetic apparatus in particular bacteria and the blue-green algae. The text and illustrations of the chapter on spores seem to show a regrettable lack of familiarity with the revealing sequence of events in sporulation set forth by Fitz-James. Although many illustrations are compelling and adequately reproduced there are many redundancies (e.g., Figs. 31c, 38a and b) and there are disturbing choices (such as Figs. 14 and 15a of chromosomes and Fig. 54 of shadowy leprosy bacilli) that do not encourage confidence in the superior resolution offered by the electron microscope. Altogether, this text has its charms as a lively, annotated scrapbook but it can not be recommended as a reliable and up-to-date guide.—*C. F. Robinow, R. G. E. Murray*

Color: A Guide to Basic Facts and Concepts by R. W. BURNHAM, *et al.;* 249 pages; $9.25; John Wiley & Sons, 1963.

The authors of this volume were members of a subcommittee of the Inter-Society Color Council charged with the task of preparing a statement of "the basic principles which should be included in any elementary teaching of color." The book is meant to be used primarily by teachers of courses on color vision who are themselves not experts in the field, and by students who have already taken such courses. Many aspects of color are considered, among them the physics of light, the visual system, normal and abnormal color vision, photometry, colorimetry, and experimental color aesthetics.

One novel feature of this book is its strict outline form. The text consists literally of series of numbered statements, most of them only one sentence long. The outline form presumably was intended to aid the reader in finding specific points of interest and to show him more clearly the hierarchical interrelation of a large variety of facts and concepts. In both respects, this reader found, paradoxically, that this form of organization is inferior to the more conventional paragraph form, particularly when generous subheadings are employed.

The authors' explicit intent was to confine themselves to the established facts of color vision, and to related definitions and terminological conventions. They avoid conjectures, speculations and hypotheses almost completely. Theories of color vision are dealt with, but only in nine short pages relegated to a section entitled "Marginal Facts." Some readers may well feel that "facts" cannot be profitably divorced from "theories," and that terminological definitions devoid of historical and theoretical content often appear unduly arbitrary.

Nevertheless, this concise and comprehensive survey of color vision will be most useful to teachers and beginning students of the subject, particularly the former. Its value is greatly enhanced by a profusion of tables, figures, and references to readily available primary and secondary sources.—*Jacob Nachmias*

Biographical Memoirs of Fellows of The Royal Society, 1963, Vol. 9; 321 pages; $6; The Royal Society, London, W.1.

This most recent volume underscores the great losses to science which have

recently taken place. Niels Bohr, Charles Galton Darwin, Ronald Aylmer Fisher and Otto Maass, our Canadian neighbor, are among those of international fame whose passing away makes modern physics, statistics, genetics and physical chemistry the poorer. The reviewer records, in addition, losses personal to himself: W. T. Astbury in fiber crystal analysis, James I. O. Masson a contemporary in physical chemistry, who went on to a Vice-Chancellorship at Sheffield, M. W. Travers, collaborator, at the turn of the century, of Ramsay in the discovery of krypton and biographer of Ramsay, who spent long fruitful years subsequently, at Bristol, until his ninetieth year. The memoirs have an eminent panel of writers, including J. D. Bernal, Sir John Cockcroft, Sir George Thomson, E. A. Flood, a colleague of Maass, E. L. Hirst, Sir John Boyd, C. E. H. Bawn, with a final memoir by Lord Adrian of the noted historian, George Macaulay Trevelyan.

Problems in Quantum Mechanics by V. I. Kogan & V. M. Galitskiy; (Translated by Scripta Technica, Inc.); 369 pages; $11.35 trade edition; Prentice-Hall, 1963.

Almost every one of the 164 problems in this book are substantial enough to require several hours of effort. The level of difficulty is that of a first, serious, year-long, physics course in quantum mechanics—that generally given to senior or first year graduate students in physics departments. Topics are in nonrelativistic quantum mechanics and include operators, barrier penetration, matrix mechanics, addition of angular momentum, time-dependent perturbation theory, W. K. B. method, atoms and molecules, collisions and isotopic spin. The present problem set is quite similar to the other Russian quantum mechanics problem book, I. I. Gol'dman and V. D. Krivchenkov (Pergamon Press, 1961), both volumes being a worthwhile addition to a text such as Landau and Lifshitz (Pergamon Press, 1958).—*Leland C. Allen*

Biology of Birds by Wesley E. Lanyon; 175 pages; \$3.95; The Natural History Press of Doubleday & Co. for the American Museum of Natural History, 1964.

The amateur whose interest in birds is evolving beyond the stage of field identification will find this small book a stimulating introduction to avian biology. Although not intended as a textbook, at least at the college level, the high school science teacher could readily utilize it as the core for an introduction to vertebrate biology.

The author, a curator at the American Museum of Natural History, has drawn heavily on the material presented in that institution's superb Sanford Hall of the Biology of Birds, but he has also updated information and examples where necessary. Covered are such topics as the origin and evolution of birds, classification, flight, migration, distribution, anatomy, physiology, vocalization, ethology, and population dynamics. These topics are illustrated by sixty-four line drawings. The text is brisk and uncomplicated, but interesting and accurate.

Biology of Birds is the best of the introductory books yet written and may be recommended without qualification to the audience for which it is intended.—*Raymond A. Paynter, Jr.*

The Chemistry of Nucleosides & Nucleotides by A. M. Michelson; 622 pages; \$18.50; Academic Press, London, 1963.

Published work on the chemistry of nucleotide derivatives is now so extensive that reviews are becoming increasingly important for following research in this field. Several excellent, though specialized, books have been written for this purpose and, with reservations, Michelson's volume can be classed with them.

After a brief introduction, there are three chapters on the chemistry of nucleosides, nucleotides, and nucleoside polyphosphates including coenzymes. The isolation, proof of structure, and properties of the substances are described, followed by a critical evaluation of the methods used for their synthesis. Most attention is given to the synthetic problems and reasonably complete discussion of the methods and their chemical mechanisms is presented. The pathways for biosynthesis of nucleotide derivatives are briefly considered in Chapter 5, which also includes detailed consideration of acylation and alkylation by phosphate esters and anhydrides, in order to suggest general mechanisms for the enzymic reactions. These considerations are a valuable summary; the author, here, limits documentation to other reviews. The remaining half of the book takes up nucleic acids, including chemical synthesis, organic and physical chemistry, and informational properties of these polymers. The physical chemistry chapter is somewhat superficial, and the last chapter, on biology, is incompletely documented and already obsolete.

The scope is broader and the chemical sections more extensive than previous reviews of this subject. The book contains citations to about 2000 publications which are discussed in the text; this is more than others though not exhaustive. The organization is adequate and the book is well written. Extensive use of structural formulae and reaction diagrams are helpful, though more labeling of these would be appropriate.

The book includes a polemic against the work of H. G. Khorana and his collaborators. Petty, unreasonable, or simply invalid criticisms are brought against Khorana's work throughout the synthetic sections. Whatever the provocation, the author degrades himself by such attacks against work which must be recognized as significant. Science thrives on alternate approaches and it seems to this reviewer that both Michelson and Khorana have successfully exploited different approaches to the synthesis of a large number of nucleotide derivatives; however, the necessary recognition of alternates has not occurred. Further, Michelson repeatedly cites unpublished work in supporting his synthetic methods, making assessment of their validity difficult

since experimental details are not given. Occasional premature generalization and misinterpretation of the results of others occur. Finally, the Subject Index and Table of Contents are inadequate for a book of this sort.

Despite some severe faults, this book should be useful as a reference in the expanding field of nucleotide chemistry, especially since no other work of comparable scope is available. It will become obsolete in about two years and the author should consider bringing it up-to-date and correcting the shortcomings.—*David B. Straus*

Friedel-Crafts & Related Reactions, Volume I, edited by GEORGE A. OLAH; 1,031 pages; $29.50; John Wiley & Sons, Interscience, 1963.

This book is the first of four volumes planned to encompass "all electrophilic organic reactions catalyzed by electron-deficient compounds-Lewis acids-whether these are molecules or cations, and include such reactions as are likewise catalyzed by those Proton Acids which are strong enough to act somewhat like Lewis Acids." The foregoing quotation taken from the Editors Preface accurately describes the scope which the Editor and contributing authors have undertaken and further indicates the variety of chemical reactions covered by the term, "Friedel-Crafts Reaction."

The initial chapter deals with the historical development of the Friedel-Crafts Reaction and is followed by a chapter which discusses the modern definition and scope of the reaction.

In order to appreciate fully the diversity of the reaction, an understanding of the concepts of Proton Acids and Lewis Acids is mandatory. An erudite discussion of these concepts comprises the third chapter.

Catalysts and solvents and the interactions between these are thoroughly considered in Chapter Four. This is probably the most important chapter in the initial volume since these factors provide the common denominator by which many reactions can be classified under "Friedel-Crafts Reactions." In-

teractions and complexes are presented in detail sufficient to satisfy all but the most exacting scholars in chapters five through eight.

The remaining chapters in Volume I deal primarily with the physico-chemical aspects of the reaction and furnish a very satisfying explanation for most of the observations reported to date.

Each chapter is adequately referenced and provides ready access to the original literature sources. Although the studies of a large number of workers are included in each chapter, the contributing authors have managed to correlate and present their findings understandably and logically.

Volume I (and probably the series) is not to be used as a textbook but rather as an encyclopedia in an important area of organic chemistry.—*B. F. Landrum*

Suicide: A Sociological & Statistical Study by LOUIS I. DUBLIN; 240 pages; $8; The Ronald Press Co., 1963.

Some thirty years ago Dr. Dublin, a life insurance statistician, published *To Be or Not To Be—A Study of Suicide*, which proved one of the few viable books on the subject worth retaining on one's shelves over the years. Now he has brought his study up-to-date.

The book opens with an examination of suicide rates in various populations, i.e., of the categories of people who commit suicide, and then proceeds to evaluate the available material on the environmental setting of suicide, such as economic conditions and war. There follow sections on the history of suicide, on its psychological aspects, such as the personality types involved, and finally a survey of efforts to prevent suicide.

Dr. Dublin's expertise is in epidemiology, and it is here that one finds the meatiest material of the book. The statistical tables, some not easily available until now, communicate with a rare clarity, and they are interpreted judiciously. A number of the relationships revealed suggest exciting hypotheses for research.

The psychological material, while breaking no new ground, is a balanced presentation of recent psychiatric thought. The historical and legal material appears to be accurate and scholarly. The delicate problem of the role of religion is handled with tact and understanding.

It is in the treatment of sociological variables that the general soundness of this book is most evident. Beginning with Durkheim, attempts to treat suicide on a purely sociological level have foundered mainly because of the psychological self-selectivity of certain sociological categories, such as the divorced and the social isolate. Dublin eschews rigid sociologism and shows awareness of the subtle interaction of sociological and psychological factors.

Finally, one notes with gratitude that there is little of academic jargon. The specialist will find valuable material in this book, the non-specialist will find it a clear, comprehensive, and informative treatment of the baffling problem of suicide.—*Maurice L. Farber*

Principles of Zoological Micropalaeontology, Vol. 1 by V. POKORNÝ; 1st English translation by K. A. ALLEN & J. W. NEALE of the 1958 German edition; 652 pages; $17.50; The Macmillan Co., Pergamon, 1963.

Ten years ago *Zaklady zoologicke Mikropaleontologie*, a one volume book by Dr. Vladimiŕ Pokorný of Karls-Universitat in Prague, was published in Czechoslovakia. Demand in Europe for this book was so great that soon after, in 1958, a German translation appeared. The German edition included several revised sections and appeared in an expanded two volume set. Now the English translation of Volume I is available.

It is primarily a text for micropaleontologists who study Foraminifera, for well over half the book is concerned with this group of protistids. The chapters on Radiolaria, Thekamoebae, Tintinnina, Incertae sedis, Chitinozoa, and Hystrichospheres and similar microorganisms although very short and unequal in depth of treatment do give a sufficient introduction to these taxonomic groups.

The book includes a thorough discussion on methods of collection, preparation, and study of microfossils. The chapter on microstratigraphical methods leaves something to be desired, however. North American geologists will be puzzled to find the Mississippian and Pennsylvanian used as examples of geologic epochs. This chapter circuitously refers to methods of paleontological correlation but never quite gets to an exemplary analysis of the various methods used by paleontologists.

This book will be prized most for its thorough treatment of the Foraminifera. Students will appreciate the fine summary on the structure, morphology, and reproduction of the Foraminifera. The sections devoted to ecology and paleoecology give an adequate introduction into this complex area. The foraminiferal classification presented is that of Pokorný and, no attempt is made to compare it with that of Cushman, Galloway, or Glaessner. This is not a serious defect, however, because of late there has been much turmoil and revision in foraminiferal taxonomy. And, the end is not in sight. Workers in micropaleontology will welcome the glossary on English—German—French—Russian synonyms of descriptive terminology.

Although the delay in translating the German edition into English has somehow prevented inclusion of an up-to-date bibliography, advanced students of micropaleontology will find Dr. Pokorný's book a handy reference and a valuable guide in their work.—*Richard K. Olsson*

Biochemical Problems of Lipids, Vol. I, edited by A. C. FRAZER; 474 pages; $23.50; American Elsevier, 1963.

Biochemical Problems of Lipids, edited by A. C. Frazer, is a compilation of fifty-six papers presented at the Seventh International Conference on Biochemical Problems of Lipids, in Birmingham, England, July 1962. The main theme of the meeting, absorption of lipids from the intestine, was subdivided into six approximately equal sessions: (1) Intraluminar Aspects of Fat Absorption, (2) Structural Aspects of the Intestinal

Cell and Absorption Studies, (3) Enzymatic Aspects of Intestinal Lipid Metabolism, (4) Chylomicrons and Lipoproteins, (5) Mitochondrial and Other Aspects of Lipid Metabolism, and (6) Wider Aspects of Lipid Metabolism.

It is somewhat regrettable, as pointed out by the editor, that publication was delayed because the authors were slow in submitting their proofs. Work of this type, which is the research vanguard of a particular field, should be made available as soon as possible. A marvelous demonstration of disseminating information is found in the discussion of thinlayer chromatography by A. F. Hofmann. He cites the fact that, at the previous symposium, two years earlier, this technique was not mentioned, yet it is currently one of the prime tools of lipid chemistry.

One minor criticism, which is familiar to readers of symposia publications, is the omission of relevant work such as Chaikoff's research on chylomicron disappearance from blood and Lack and Weiner's recent observations regarding the site of intestinal bile absorption.

As with other symposia, there is a wide scope in relation to type and quality of material discussed. While the majority of papers discuss specific research projects carried out in the authors' laboratories, there is excellent coverage of general topics such as thin-layer chromatography by A F. Hofmann and by J. G. Lines; fine structure of intestinal cells by F. S. Sjöstrand, P. F. Millington, and J. B. Finean; and lipids in mitochondrial electron transport by D. E. Green and S. Fleischer. The main contribution is the collection of detailed reports from many of the recognized authorities in lipid chemistry. It offers the opportunity for these people to disclose their latest findings and express their views in a rapidly expanding area.

Biochemical Problems of Lipids should be a valuable addition to the contemporary literature for workers in lipid chemistry and related research.—*J. Fielding Douglas*

Chemical Applications of Group Theory by F. Albert Cotton; 295 pages; \$12.50; John Wiley & Sons, Interscience, 1963.

Graduate students in chemistry are confronted with quantum mechanics, statistical mechanics, group theory, and other theoretically oriented subjects in addition to the more empirical parts of the field. A few years ago a student trying to obtain a working knowledge of one of these areas usually resorted to standard reference works or to the original literature. Unfortunately, sophisticated treatments often presented barriers to all but the most persistent even among the physical chemists. Recently, however, a noticeable improvement has been brought about by the introduction of a number of textbooks for seniors and first year graduate students. Professor Cotton's book, which originated as a one semester course at the Massachusetts Institute of Technology, falls into this category. He has attempted to introduce the chemical applications of group theory in "... as unpretentious and down-to-

earth a manner as possible," and has in general succeeded.

The book is divided into three parts. Part I (Principles), which begins with the necessary definitions, includes an exceedingly clear discussion of symmetry elements and operations. Illustrations and examples of chemical interest are consistently used to supplement the more abstract statements. Part II (Applications) makes up almost two thirds of the book and contains four independent chapters: Construction of Hybrid Orbitals, Symmetry Aspects of Molecular Orbital Theory, Ligand Field Theory, and Molecular Vibrations. In each chapter enough additional material has been introduced to give a balanced coverage. For example, resonance energy is discussed and the Hückel approximation is applied to a series of organic molecules in the chapter on molecular orbital theory. Part III (Appendices) covers some properties of determinants and matrices and contains character tables and some additional comments about the intrepretation of resonance integrals. A particularly convenient feature is the set of character tables for chemically important symmetry groups which is printed as a separate ten page booklet and enclosed in a pocket in the back of the book.

I strongly recommend this book to students and to practicing chemists who are not familiar with group theory. It is by far the most painless introduction to group theory available for the chemically oriented.—*Charles S. Johnson, Jr.*

X-Ray Studies of Materials by A. Guinier & D. L. Dexter; 156 pages; $6.75; John Wiley & Sons, Interscience, 1963.

A book bearing Guinier's name is usually an event; this one isn't. The preface indicates that this little volume is designed to tell beginning physics graduate students what X-ray scattering techniques can do. The book does it in a very concise and, unfortunately, sometimes strictly limited way. The treatment is formal, not novel, and uses

vectors throughout. Very often where it begins to get interesting the discussion is abruptly terminated. There is repeated evidence that this English translation has been rather carelessly put together. It suggests that not enough care has been used in producing even such a small book. I believe the book misses its point; it is too concise for the beginning graduate student for whom it was intended and it is certainly not sufficiently detailed for the specialist.

Almost everything in this book can be found more fully treated in other books available in English. To be specific, this book has nine chapters including a brief introduction. Seven pages are devoted to the production and detection of X-rays; six pages to "X-Ray Images" including microradiography and X-ray microscopy. The major part of this book (chapters 4, 5, 6, 7, 8) deal with various aspects of X-ray diffraction, theory, experimental methods, determination of structure, crystallite size determination, small angle scattering, etc. However, no full treatment of any of these is given. The final chapter, a brief one, is devoted to X-ray spectroscopy.—*I. Fankuchen*

Nuclear Shell Theory (Volume 14 of Pure & Applied Physics Series) by AMOS DE-SHALIT and IGAL TALMI; 573 pages; $14.50; Academic Press, 1963.

Angular momentum techniques are basic tools of nuclear and atomic physicists. These techniques are thoroughly described in *Nuclear Shell Theory* by de-Shalit and Talmi and are worked out in detail for a number of examples relevant to nuclear shell-model calculations.

The authors' preface states: "The book is intended for graduate students who have had a course in elementary quantum mechanics." This reviewer feels, however, that the subject matter is too specialized and detailed for the usual graduate student interested in this type of theoretical physics. Several competing texts (including two in the same series of monographs) have appeared in recent years and are considerably cheaper and more concise.

The practicing physicist involved in calculations of nuclear (or atomic) spectroscopy, however, will find this book to be of great value. Intelligible descriptions are given of many of the modern methods of shell-model calculations which have been heretofore available only in scattered research papers. Of particular value is a series of appendices containing definitions, formulae, and tables needed in nuclear shell-model calculations. This sixty-page collection, by itself, would justify the acquisition of the book by the practicing shell-model physicist.—*Ernest Rost*

Fracture of Solids, edited by D. C. DRUCKER & J. J. GILMAN; 708 pages; $28; John Wiley & Sons, Interscience, 1963; (Proceedings of International Conference, Maple Valley, Wash., August 1962; Vol. XX of Metallurgical Society Conferences).

This collection of papers has four divisions, namely, on Continuum Mechanics, Microstructural Phenomena, Atomistic Mechanisms and Environmental Effects. Each division starts with a general review—by D. C. Drucker (48 pp., 54 references), J. R. Low (40 pp., 116 references), J. D. Meakin and N. J. Petch (23 pp., 71 references) and A. R. C. Westwood (53 pp., 180 references) and contains from four to seven additional research reports.

It is obviously impossible to comment here on each contribution and it would be unfair to deal with one or two. Just one observation may be recorded here: several authors point out that the surface energy criterion of crack propagation is no good; and still they use it! The general impression is that the field is almost too wide even for a book of over 700 pages. The classification chosen by the editors may be supplemented by other systematics, such as—brittle solids, viscoelastic solids, etc., or single crystals, polycrystalline solids, etc., or metals, polymers, and so on; and data will be found in the volume for each of these classes. Thus, although the quality of the individual papers

naturally is not uniform, the amount of information and the wealth of ideas found in the book make this a valuable addition to any library of science or engineering. As is usual in conference proceedings, no author or subject index is provided; the latter would have been welcome.

The printing and binding are very good; there are many photographs reproduced in the text; but is this a sufficient justification for the price which might frighten away many students who would have so much profited from the book?—*J. J. Bikerman*

A Dictionary of Zoology by A. W. LEFTWICH; 290 pages; $6; Van Nostrand Co., 1963, and *Collegiate Dictionary of Zoology* by ROBERT W. PENNAK; 583 pages; $8.50; The Ronald Press Co., 1964.

While it is certainly easier to review a dictionary than to write one, one has the difficulty that they are impossible to read from cover to cover. However, one can examine sections, check them against other dictionaries and try out various other experiments for meaning and comprehensiveness.

On both scores, Pennak's book rates very high, while the more modest dictionary of Leftwich seems inadequate. In the definitions themselves, Dr. Pennak gives the essential points and the qualifications. Secondly, by being very much larger, the chances that the word is defined are excellent in the *Collegiate Dictionary*. This includes both anatomical and descriptive terms as well as nomenclature. Dr. Pennak's *Dictionary* can be very highly recommended.—*J. T. Bonner*

The Physiology of Earthworms by M. S. LAVERACK; 206 pages; $7; The Macmillan Co., Pergamon, 1963. (Vol. V, International Series of Monographs on Pure & Applied Biology, G. KERKUT, General Editor).

This is a very welcome book, for it brings together all the more important work that has been done on one of the most readily and universally available of laboratory animals. (Long before the

age of the airplane, earthworms had become true internationalists.) Furthermore, as Charles Darwin went to infinite pains to show, earthworms play an important role in the economy of nature. Laverack places the emphasis, quite rightly, on work done since the publication of Stephenson's *Oligochaetta* over 30 years ago. Compared with that monumental classic, this volume seems slim. Its field is far more restricted but the restriction itself achieves a focus appropriate for our times. Even so, the book covers a wide area: Biochemical Architecture, Digestion, Metabolism, Nitrogen Excretion, Water Balance, Respiration, Regeneration, Neurosecretion, the Nervous System, Behavior, and much else.

The style is clear and informative, and the material almost always up-to-date and accurate. The non-specialist will be interested to learn that earthworms differ greatly from flatworms in ability to "remember" after regeneration what they had learned previous to surgery. Like planarians, earthworms can remember the correct turn in a T-maze after removal of the brain or even of the first five segments. But, in marked contrast to what is said to be true of flatworms, after the regeneration of a new brain, the learning is lost. Apparently an earthworm can be born anew as innocent as the day it hatched from its egg cocoon.

The presentation of regeneration is marred by several inaccuracies. It is not true that "oligochaetes are able to regenerate both anterior and posterior ends with almost equal facility." The assertion that earthworms can regenerate better in some seasons of the year than in others does not hold, at least for Baltimore earthworms. While alkaline phosphatase may be important in regeneration, the work quoted, which was done in our laboratory, gives scant support to such an idea. The most interesting and authoritative portions of the book deal with metabolism, especially excretion, and with the nervous system, but the entire volume is a rich mine of well-organzied information. The usefulness of the book is so great that it at once becomes a "must" for every biological laboratory from high school to university. There are 58 well selected figures, an excellent although not exhaustive bibliography, and an index.—*Gairdner Moment*

The Flowering Process (International Series of Monographs on Pure & Applied Biology, Plant Physiology Division, Vol. IV) by FRANK B. SALISBURY; 234 pages; $8.50; The Macmillan Co., Pergamon, 1963.

Most of the right materials are here, with few major errors or omissions, yet this is not a particularly satisfactory book. The preface indicates what must have happened. A book first conceived for high-school biology teachers and then revised for graduate students; a book that tries to set flowering in the broadest biological context yet also offers an almost day-by-day account of work in a single laboratory; a book read and criticized, in one stage or another, by many of the foremost investigators in its field: such a book is bound to embody the contradictions and uncertainties of its origins.

The first four chapters survey various controls on flowering and their significance. Though one might question the distribution of emphasis among topics—twelve pages are devoted to demonstrating, with symbols reminiscent of the medieval herbalists, that there are at least 777 possible response types—this is a good elementary introduction. This discussion of "Ecology and the Flowering Process" is particularly valuable, considering the unconcern of many physiologists for anything outside the laboratory.

The remaining six chapters use excursions about a central theme of experiments with *Xanthium*, many the author's own, to fill in the outlines sketched previously. The courage required to undertake such a tour-de-force unfortunately did not ensure its success. While the cocklebur has been a workhorse in many photoperiodic studies it has provided relatively little primary information on temperature effects, rhythms or photoreception, to name three significant topics. Hence the

excursions, though frequently fine in themselves, often represent long digressions, and might better have been incorporated earlier. Occasional passages read like a manual of *Xanthium* experimentation, including instructions on preparing one's notebook. Yet this aspect is flawed, for example, by a less than adequate discussion of germination problems, by a rather cavalier attitude towards statistics and experimental design, and by the absence of any information on the tangled question of *Xanthium* taxonomy and identification.

It is perhaps too easy to find things wrong with a book that attempts so much. Yet since there are valuable things in it, both in information and concept, it can best be characterized as uneven, occasionally unreliable, but certainly worth reading, for beginner and expert alike.—*William S. Hillman*

General Oceanography by GÜNTER DIETRICH, translated by FEODOR OSTAPOFF; 588 pages; $20; John Wiley & Sons, Interscience, 1963 and *Ocean Wave Spectra* (Proceedings of a National Academy of Science Conference); E. C. STEPHAN, *et al.*, chairmen; 357 pages; $12.50; Prentice-Hall Publishers, 1963.

After many, many years of getting along on just Sverdrup, Johnson, and Fleming's *The Oceans*, the field of oceanography has suddenly been inundated with new books. These books range from popular accounts of the "Gee whiz" school through serious general oceanography texts to extremely specialized treatises on specific corners of the field. The two books reviewed here stand almost at opposite ends of this spectrum.

General Oceanography, by Dietrich, is a translation of his *Allgemeine Meereskunde*, published in 1957. No attempt was made to bring the material up-to-date, largely because of the soon-expected publication of the results of the IGY cruises. While the book is supposed to be a general introduction to the whole field of ocenography, it is more nearly an introduction to physical oceanography. Biological and geological oceanography get only short shrift; chemical oceanography fares only slightly better. This, perhaps, is to be expected in so short a book. The three volumes of the recently published *The Sea*, in spite of their greater space, omit most of marine biology and do less than justice to chemical oceanography.

As an introductory text in physical oceanography, this book will usually be compared (to its disadvantage) with the recent text by von Arx. Besides being an older book, and thus including few of the results of the post-Sputnik boom in oceanography, Dietrich's book is decidedly hard going in many places. This results in part from the curious arrangement of subjects. In part, however, it stems from the involved style in which the text is written. Many times I felt at first reading that Dietrich and von Arx were directly contradicting each other, only to discover after careful re-reading of Dietrich and considerable thought that they were indeed saying exactly the same thing.

Oceanographers will experience a certain feeling of déjà vu on first opening this book; most of the figures used were taken from earlier publications, notably *The Oceans*. Dietrich also leans heavily upon European sources, and particularly upon the German and Scandanavian literature. Better than half of the bibliography is in German.

Ocean Wave Spectra is a very different kind of book, addressing itself primarily to those oceanographers actively working in this field (most of whom were actually present at this conference). The material covered ranges from the completely theoretical and mathematical treatment of wave spectra to the completely practical problem of measurement of wave motion. Included with the formal papers are records of discussions after each paper and at the end of the sessions. It is evident to even the most casual reader that a fair amount of disagreement exists on almost any topic mentioned in the conference. While this is certainly not a book for the average reader, (nor even the average oceanographer), even the most determinedly non-mathematical marine biologist will find items of in-

terest tucked away between the equations and the graphs.

On the dust jacket the publishers call this book "a summary of the present state of the art in the several fields of ocean wave study." The book does not really attain this goal, largely because the authors were speaking to a select group of their co-workers, and therefore pre-supposed a large body of knowledge on the part of their listeners, knowledge that the reader should have if he wants to follow the arguments presented. The book is thus much more a statement of some problems in the field than it is a general summary. It is a beautifully designed and printed book, and surprisingly cheap as texts go these days.—*Peter J. Wangersky*

The Sea, Ideas & Observations on Progress in the Study of the Seas, Vol. III, edited by M. N. Hill; 963 pages; $28; John Wiley & Sons, Interscience, 1963.

The presumably final volume of *The Sea* is in many ways the most useful of the three. This is due in a large part to the section on geophysical exploration, which gathers together material on the newer techniques and interpretations which has heretofore been available only in scattered journals.

The volume is divided into four parts. Under the general topic of *The Earth Beneath the Sea* are listed geophysical exploration, topography and structure, and sedimentation. The fourth section, *History*, concerns itself with the origin of life and with the paleontological record. As in the other volumes in this series, the authors of each article have concentrated on the broad aspects of their topics, referring the reader to the original literature for detailed arguments and descriptions.

The editor's choice of authors was excellent; each man is an authority in his field. By the same token, however, there are no surprises to be found in this volume, since each man is reviewing work with which he has long been identified. One might occasionally wish for a fresher look at some of the phenomena described.

The quality of the writing is so consistent that it would be difficult to single out specific authors for either praise or blame. The major drawback of this volume is that the breadth of material included is so great that almost every section suffers from extreme compression. This is particularly noticeable in the last section, where all of oceanic paleontology is zipped through in some sixty pages, with thirty-five of them devoted to the Pleistocene.

This volume is as well-edited and well-produced as the two earlier volumes. The consistency of the editing in so large a work is truly remarkable. Again, however, the price will keep the book out of the hands of the people who could most benefit from it.—*Peter J. Wangersky*

Operations Research in Research & Development, edited by Burton V. Dean; 289 pages; $8.50; John Wiley & Sons, 1963.

In a period when national attention is focused on the management of research and development, this book on operations research methods is particularly timely. The twelve chapters, originally presented as papers at a 1962 Case Institute of Technology conference on research and development management systems problems, provide diversity of subject and quality.

The initial and most outstanding paper in the volume is Ellis A. Johnson's "A Proposal for Strengthening U.S. Technology." It identifies several classes of major national problems in research and development policy including a group of factors which act to lessen technical program efficiency. Johnson suggests many possible solutions including a program of university-associated National Institutes in key scientific areas, conversion of liberal arts graduates to science, coordination of multi-agency information on government-sponsored research, etc. The provocative analysis and ideas of this chapter merit wide readership by science-technology leaders.

Another interesting paper is Chapter 6 by Hertz and Carlson, which presents

simple but useful approaches to the problems of project selection, evaluation, and control. The pragmatic non-mathematical presentation by these operations researchers is in sharp contrast to the less meaningful "mathemateze" of some of their colleagues. Also especially well done is the article by Weiss on the relation of company proposal efforts to sales.

The one obvious fault in the book is its imbalanced heavy coverage of PERT (Program Evaluation and Review Technique) and associated network methods for project planning and control. Of the four articles which treat PERT and its extensions, only the Norden article combines data collection research with new concepts for organization and use of the data.

This collection of papers should suggest numerous ideas to the manager of research and development. The book is an important addition to the growing literature on the use of analytical methods in this field.—*Edward B. Roberts*

Mass Spectrometry of Organic Ions, edited by F. W. McLafferty; 730 pages; $24; Academic Press, 1963.

The title of this book is unfortunate in that while the overall bias of the book is towards the organic chemist, it does contain much that is of interest to the physical chemist. In fact, the first eight of the thirteen chapters are of general interest. Chapters 1, 7 and 3 cover the quasi-equilibrium theory of mass spectra, the decomposition and rearrangement of organic ions and the appearance potential data of organic molecules, while chapters 2, 4, 5, 6 and 8 cover ion-molecule reactions, negative ions, free radicals, ions from discharges and flames, and high resolution mass spectrometry.

The first eight chapters serve as an admirable introduction to the extent to which mass spectrometry can assist the organic chemist in the elucidation of the structure of complex organic molecules. Examples of this assistance are given in the last five chapters which present detailed reviews of the mass spectrometry of long chain esters, alkyl benzenes, terpenes, petroleum, and natural products such as alkaloids, amino acids and steroids.

The ten page index is disappointing since it is largely composed of entries from the last four hundred pages; for example, under D only two of the eighty odd entries come from before page 300. It is to be regretted that there are no index entries referring to the negative ion spectra on pages 176, 179 and particularly 195 ff, or to the different types of mass spectrometer, to quote two examples.

The book is well-produced and is a worthy addition to the literature on mass spectrometry. The reviews are comprehensive and up-to-date and the book can be recommended to those using or interested in using mass spectrometric techniques.—*Graham S. Pearson*

INDEX TO BOOKS REVIEWED. *This index is for quick reference to books reviewed in this issue. In the light of the pressure on space in THE SCIENTISTS' BOOKSHELF section, reviewers are urgently requested to bear in mind that, normally, the length of reviews should be about 300 words*

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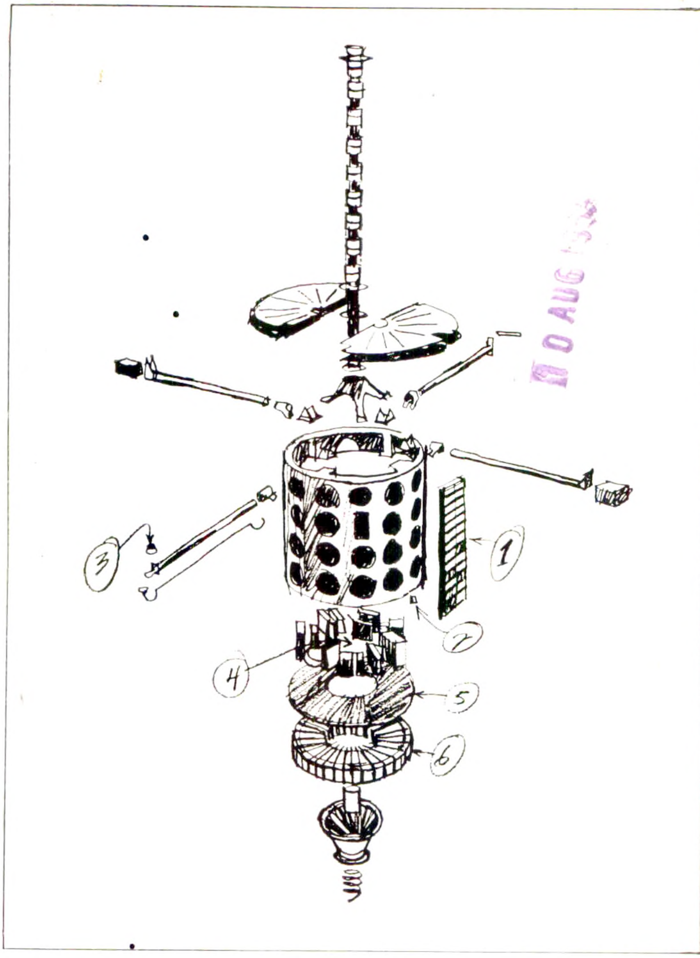

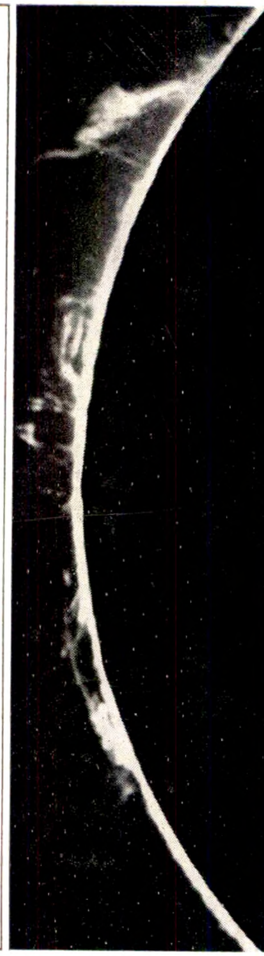

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AMERICAN SCIENTIST

SEPTEMBER, 1964

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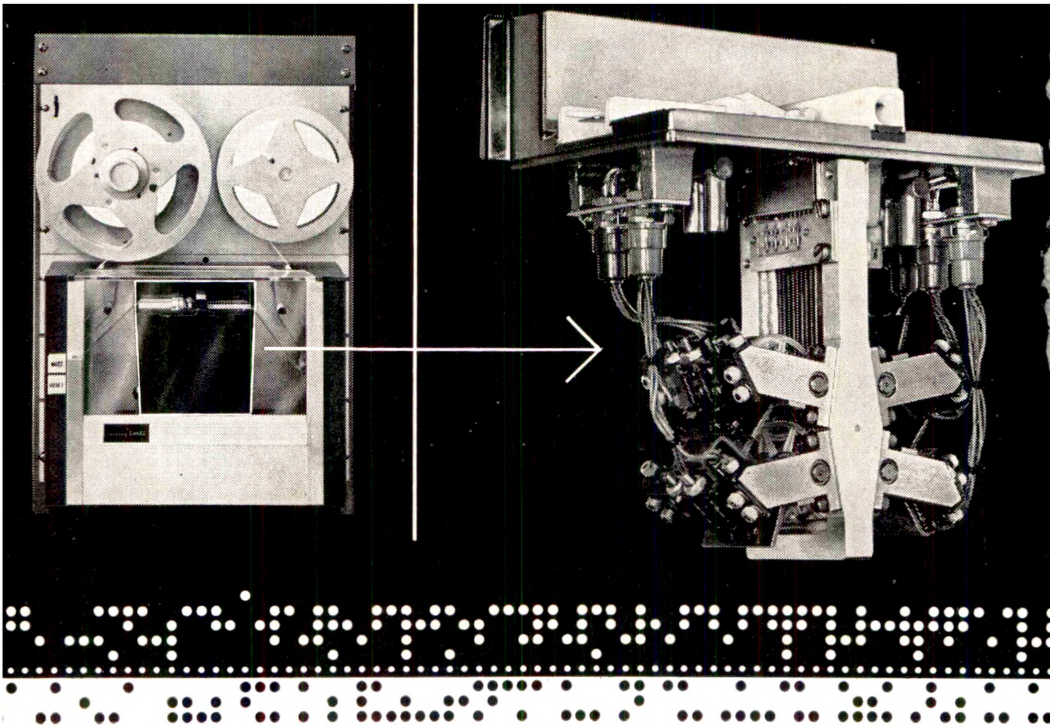

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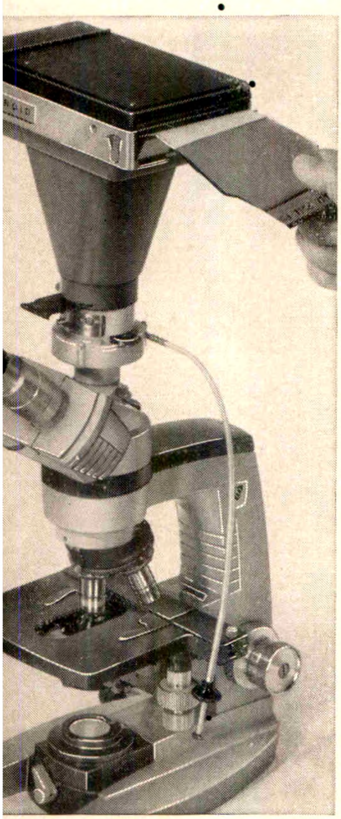

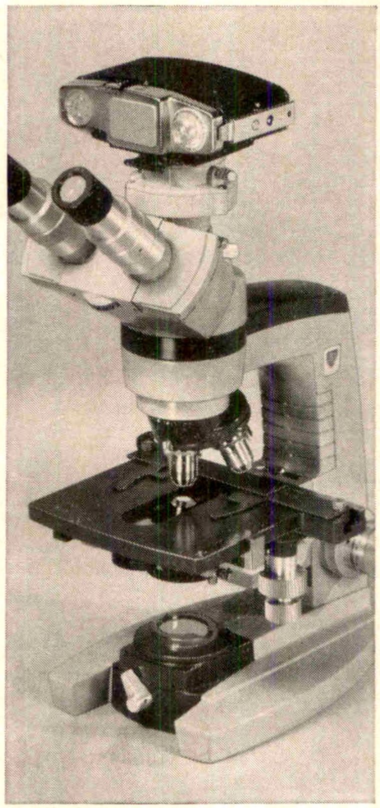

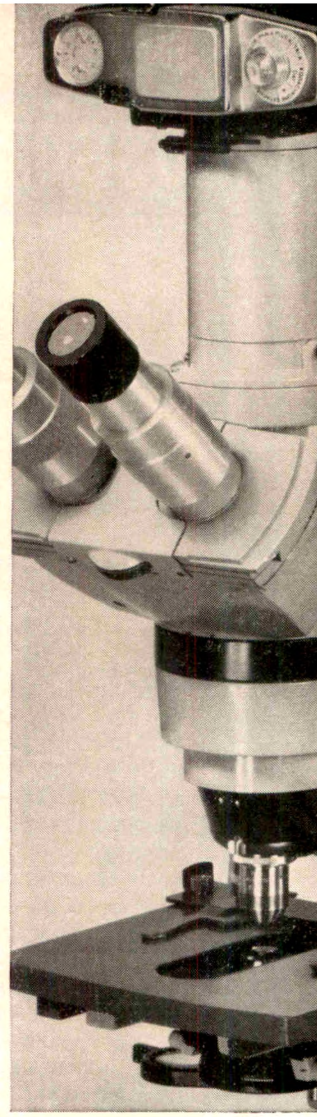

AMERICAN SCIENTIST

VOLUME 52 SEPTEMBER 1964 NUMBER 3

PUBLISHED IN THE INTEREST OF SCIENTIFIC RESEARCH
AT TWENTIETH AND NORTHAMPTON STREETS, EASTON, PENNSYLVANIA

CONTRIBUTORS

This National Sigma Xi-RESA Lecture was given by Dr. Simon in April 1964. Chess players and solvers of geometric problems, among others, can learn from it what processes are involved in reaching the goals of Checkmate and Q.E.D. The joint paper with Dr. Newell, stemming from the lecture, shows how information processes, human and artificial, can be organized to accomplish difficult tasks. The neurophysiological mechanisms in man and electromagnetic or electronic devices in the "black boxes" are not essential elements in formulating the science of information processing. Dr. Simon is Professor of Administration and Psychology and Associate Dean in the Graduate School of Industrial Administration. Dr. Newell was a research scientist with the RAND Corporation and is now Institute Professor of Systems and Communication Sciences. Both men are at Carnegie Institute of Technology, Pittsburgh, Pennsylvania.

What happened to airborne fission products during the moratorium on nuclear testing, 1959–1961, is here set forth by Dr. Lockhart in a sequel to an article in AMERICAN SCIENTIST, September 1959, by Lockhart and King. The author is Head of the Physical Chemistry Branch of the U.S. Naval Research Laboratory, Washington, D.C. He has monitored the distribution of natural and fission product radioactivity along the 80th meridian from Greenland to Chile since 1958, through the moratorium, during renewed testing, and into the era of the present test-ban treaty.

AMERICAN SCIENTIST is published quarterly (March, June, September, December) by the Society of the Sigma Xi at the Mack Printing Co., 20th and Northampton Sts., Easton, Pa.

Second-class mail privileges authorized at Easton, Pa. Additional entry at the New Haven, Conn. post office, December 9, 1943. This publication is authorized to be mailed at the special rates of postage prescribed by Section 132.122.

STERLING B. HENDRICKS, *Salt Transport Across Cell Membranes* 306

Dr. Hendricks, "scientist" in the Federal Civil Service, gave National Lectures on two interdisciplinary subjects in which he works. He selected the one on salt transport for publication as illustrative of the interplay between physical and biological disciplines. This subject, as well as "biological timing," the other lecture, is in best accord with the integration of many approaches to biology expressed by G. Gaylord Simpson and Clifford Grobstein in the 75th Jubilee number of AMERICAN SCIENTIST. Address: Mineral Nutrition Laboratory, U.S. Department of Agriculture, Beltsville, Maryland.

G. EVELYN HUTCHINSON, *The Lacustrine Microcosm Reconsidered* 334

Professor Hutchinson of Yale University's Department of Biology, who has written so many fine essays on an impressively wide range of subjects, writes here on ecology, his own special area of interest. The address, given after the transfer of keys of the Laboratory of Limnology from the National Science Foundation to the Board of Regents of the University of Wisconsin in May 1964, is here reproduced for our readers. It spans the years from Darwin to present day researches by the author and his students.

LAWRENCE B. SLOBODKIN, *The Strategy of Evolution* 342

A Professor in the Department of Zoology, University of Michigan, Ann Arbor, Michigan, Dr. Slobodkin is one of the leading ecologists concerned with problems of populations, evolution, and stability. These are the topics treated in his book *Growth and Regulation of Animal Populations*. A Ph.D. of Yale University in 1951, with research experience in the Fish and Wild Life Service and in Oceanography, he has been in Michigan since 1953. In this original essay the author presents some important new thoughts concerning his major field of interest.

ARTHUR V. TOBOLSKY, *Polymeric Sulfur and Other Polysulfide Polymers* 358

The experiments on sulfur, familiar to every high school chemistry student, whereby the element melts to a pale yellow liquid, darkens and becomes very viscous at higher temperatures, and, when thrown into water, yields a plastic form of sulfur is here given a sophisticated and unified treatment by a professor of chemistry in Princeton University. The diverse facts are interpreted by a single cohesive treatment which led to verified predictions concerning not only elastic sulfur but also plastic materials containing sulfur and polysulfides. Address: Frick Chemical Laboratory, Princeton, New Jersey.

BARRY COMMONER, *DNA and the Chemistry of Inheritance* 365

Dr. Commoner is Professor of Plant Physiology and Chairman of the Executive Committee of the Botany Department at Washington University, where he also serves as a member of the Committee on Molecular Biology. He has done extensive research on basic questions regarding the physico-chemical foundations of biological processes. He is responsible for the discovery of free radicals in biological systems, by means of the electron spin resonance technique and for a series of investigations on the biosynthesis of tobacco mosaic virus. Dr. Commoner is also active locally and nationally in matters relating to the social implications of science. He has a flair for looking at familiar situations in a new and original way. Here he gives DNA an unconventional setting in a thoughtful and vigorous essay.

Aaron Lemonick, *Essay Review of Encyclopaedic Dictionary of Physics* 389

A Professor of Physics in Princeton University evaluates, in this essay review, an Encyclopaedic Dictionary of Physics edited by J. Thewlis and associates and published, in eight volumes, by the Macmillan Company. The set contains 5600 pages. The reviewer writes about several articles in some detail, to gain a feeling for the whole, on Fundamental Particles, Quantum Mechanics, for example. He lists the articles on nuclear physics and read others at random. Though heavy on technology the dictionary should be of use to graduate and pre-graduate students in Physics and to specialists in other fields.

J. Tuzo Wilson, *Science is Everybody's Business* 266A

The Professor of Geophysics in the University of Toronto, a former Sigma Xi-RESA National Lecturer, was the guest speaker at the fifteenth birthday party of the College of Arts and Sciences, University of Buffalo, New York, April 4, 1964. With the kind permission of Dean Milton C. Albrecht of that university and of the speaker, we are pleased to reproduce this interesting commentary on the place of science in everybody's business, in the News and Views section of this issue.

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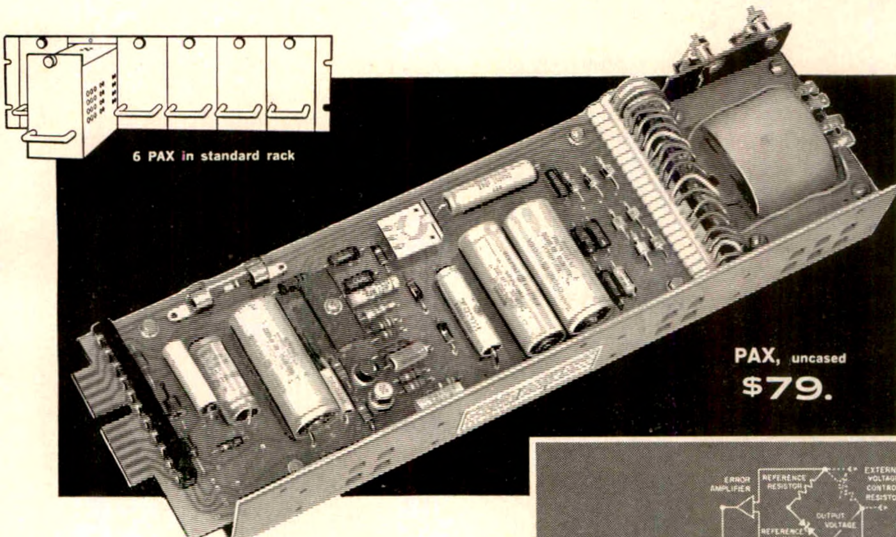

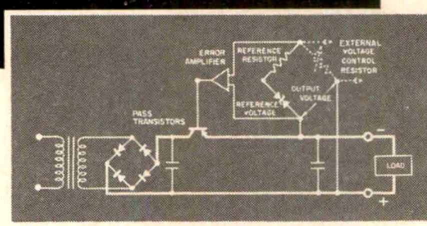

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NEWS AND VIEWS

By the Board of Editors and the Membership of the Society of the Sigma Xi and the Scientific Research Society of America, RESA

SIGMA XI-RESA GRANTS-IN-AID OF RESEARCH

Report of the Awards made by the Grants-in-Aid of Research Committee for 1964

During 1964, the Committee on Grants-in-Aid of Research received 356 applications for a total amount requested of approximately \$350,000. Although a record high of \$100,000 was available for awards, the Committee was faced with the task of not only rejecting many requests which it felt most deserving—it was unable to review approximately 84 of the applications submitted. This problem was explained in detail on page 280 of the June issue of the AMERICAN SCIENTIST.

Of the 272 applications acted upon—207* were granted a total of \$102,372—this amount representing approximately 60% of the total requested on these applications. The 203 awards totaling \$99,942 actually received are listed in the following pages.

Of special interest are awards #67 and #158. These were made possible by earnings of the Hsien Wu and Daisy Yen Wu Fund. Again, this year, the Fund was increased by an additional gift of \$6,172.50 from Mrs. Daisy Yen Wu in memory of her husband, Dr. Hsien Wu.

The Committee is pleased that the Grants-in-Aid of Research activity has received the support which has enabled it to increase ten-fold in ten years, but it is disturbed by its inability to satisfy, at least in some part, those applications which most certainly could qualify for assistance if additional funds were available. The Executive Committee will be asked to consider the long-range aspects of this activity at its meeting this fall.

Harlow Shapley, Chairman,
Grants-in-Aid of Research Committee.

The awards granted and accepted are:

1. George A. Agogino, Eastern New Mexico University. "The casting of Paleo-Indian points from the Clovis and Folsom horizons at the Blackwater Draw site locality No. 1"; \$400.
2. Roger A. Anderson, University of Colorado Museum. "Lichen Flora of Rocky Mountain National Park, Colorado"; \$500.
3. Kenneth K. Asplund, University of California, Los Angeles. "The systematics and ecology of *Cnemidophorus maximus* (Teiidae) in Baja, California"; \$300.
4. Theodore Axenrod, The City College of New York. "Conformational aspects of biradical intermediates"; \$400.
5. Lawrence H. Balthaser, Indiana University. "A sedimentological and paleoecological study of middle Chester strata in southern Indiana"; \$200.
6. John C. Barnes, Queen's College, St. Andrews University, Dundee, Scotland. "Solvation phenomena in inorganic complexes and formation con-

The attention of the membership is called to the fact that **ALL** correspondence with respect to Grants-in-Aid of Research should be addressed to the Executive Secretary, Professor T. T. Holme, at 51 Prospect Street, New Haven 11, Conn.

* Four of these subsequently did not accept the awards.

stants and redox potentials of complexes and their relation to charge transfer spectra"; $425.

7. Raymond D. Barnes, University of California, Davis. "The relative importance of fetal versus maternal mechanisms in the initiation of parturition"; $110.

8. Delia M. Barreto, University of Oxford, England. "The study of the importance of stromal abnormalities in the modification of red cell shape"; $500.

9. James H. Barrow, Jr., Hiram College, Ohio. "Studies of immune responses in poikilothermic vertebrates"; $375.

10. Peter N. Bartlett, University of Colorado. "Population structure and energy intake of *Sceloporus occidentalis*"; $275.

11. Suzanne W. T. Batra, University of Kansas. "The behavior and ecology of sweat bees (Halictini) of India"; $692.

12. Laurence E. Bayless, Tulane University. "Ecological relationships between symatric populations of *Acris gryllus* and *A. crepitans* (Amphibia; Anura) in southeastern Louisiana"; $400.

13. Albert J. Beck, University of California, Davis. "Seasonal movement patterns of *Myotis yumanensis*"; $325.

14. James B. Benedict, University of Wisconsin. "Solifluction movement rates"; $175.

15. Patricia R. Bergquist, University of Auckland, New Zealand. "Examination of Lendenfeld's material to commence a revision of the Keratosa utilizing modern systematic techniques"; $1000.

16. Sarah A. Bernhardt, University of Massachusetts. "A year round investigation of the fauna associated with Sphagnum moss in several ecological situations and dealing primarily with the Arthropods"; $550.

17. William E. Bickley, University of Maryland. "Mosquito behavior"; $400.

18. Paul H. Biebel, Dickinson College. "Morphology and cytology of zygospore germination in Saccoderm Desmids"; $1000.

19. Carman A. Bliss, University of Southern California. "Investigation of the glycoside from *Mentzelia laevicaulis*"; $500.

20. Carol A. Bocher, Bryn Mawr College. "Studies in lens induction in *Fundulus heteroclitus*"; $425.

21. Digamber S. Borgaonkar, University of North Dakota. "Genecological studies on the Agropyrons"; $400.

22. Zeddie P. Bowen, University of Rochester. "Brachiopoda of the Keyser and Helderberg formations of Pennsylvania"; $400.

23. Branley A. Branson, Kansas State College. "Comparative cephalic and appendicular osteology of the fish family Catostomidae. Part II. *Ictiobus*"; $375.

24. Douglas C. Brew, Cornell University. "Stratigraphy and paleontology of the Naco formation of central Arizona"; $400.

25. Jerome Brezner, State University of New York at Syracuse. "Molecular heterogeneity and specificity of insect glycolytic enzymes"; $275.

26. Douglas G. Brookins, Kansas State University. "Rb-Sr geochronological investigations of some peridotite plugs, Riley County, Kansas"; $450.

27. Edwin H. Brown, University of California, Berkeley. "Petrology of greenschist facies rocks in Eastern Otago, New Zealand"; $800.

28. Hannes K. Brueckner, Yale University. "Stratigraphy, structure, and petrology of metasediments and gneisses covering bulk of Tafjord area, at the eastern tip of Storefjorden in west-central Norway"; $200.

29. James E. Bugh, Western Reserve University. "Geomorphology of the Sangre de Cristo Mountains, San Miguel County, New Mexico"; $250.

30. James A. Cain, Western Reserve University. "Quantitative petrology of the red rapakivi granites of northeastern Wisconsin"; $300.

31. Howard W. Campbell, University of California, Los Angeles. "Thermoregulatory mechanisms in the snake, *Masticophis flagellum*"; $300.

32. Raymond A. Cappellini, Rutgers University. "The role of zinc in the synthesis of cytochrome C in the smut fungus *Ustilago sphaerogena*"; $600.

33. Karl J. Casper, Western Reserve University. "Beta-gamma angular correlations from excited nuclei using high resolution solid state detectors"; $675.

34. Charles J. Cazeau, State University of New York at Buffalo. "Sedimentary petrology of pleistocene glacial deposits of Erie County and vicinity, New York"; $500.

35. Samuel J. Ciurca, Jr., Museum of Petrified Wood, Rochester. "Search for land plants from the Silurian of New York State"; $400.

36. Wilbur B. Clarke, Southern University. "Preparation and infrared study of metal chelates of N-benzoyl-N′-benzenesulfonylhydrazides with relation to the McFadyen-Stevens reaction"; $1200.

37. Russell E. Clemons, University of Texas. "Geology and structural interpretation of the Chiquimula area, Guatemala"; $500.

38. James A. Clifton, University of Kansas. "A diachronic study of the Potawatomi Drum Cult. Analysis of the origin of and stability and change with a nativistic cult movement among the Prairie Potawatomi, and of the functions of the Cult in contemporary Potawatomi culture"; $700.

39. Kent C. Condie, University of California, San Diego, "Petrology and geochemistry of the late Precambrian rocks of the northeastern Great Basin"; $200.

40. Paul P. Cook, Jr., Seattle University. "A re-evaluation of the subfamiliam, tribal, and generic relations in the family Membracidae, based on an investigation of the male genitalia. (Insecta; Homoptera)"; $500.

41. John D. Corbit, University of Pennsylvania. "Intravenous feeding and drinking in the rat"; $700.

42. Donald J. Crowley, Brown University. "The paleoecology of a marine algal bank in the Wyandotte formation (Upper Pennsylvanian) of eastern Kansas"; $500.

43. Harold A. Curran, University of North Carolina. "Conodonts from the Lincolnshire formation (Middle Ordovician) of Virginia"; $250.

44. Michael D. Dahlberg, Tulane University. "A systematic review of the menhaden (Brevooria sp.) of the Gulf Coast of the United States"; $300.

45. Donald W. Davidson, University of Alabama. "The effects of two contrasting light regimes on growth of six shrub species from the upland forests of northern New Jersey, as reflected in leaf anatomy"; $425.

46. Patricia J. DeCoursey, Washington State University. "Preparation of 5 manuscripts from completed experiments for scientific publication"; $1000.

47. Donald G. DeLisle, Simpson College, Indianola, Iowa. "Cytotaxonomic studies of the North American species of Aristida (Gramineae)"; $400.

48. George H. Denton, Yale University. "Glacial geology of the northeastern border of the St. Elias Mountains, Yukon Territory, Canada"; $200.

49. Sister Mary Howard Dignan, Clarke College, Dubuque, Iowa. "Stabilization of ego identity in college students over a two year period"; $400.

50. Vincent DiLollo, Indiana University. "The Amsel 'Frustration Effect' and its relation to adaptation level"; $600.

51. James R. Dixon, New Mexico State University. "The systematics and distribution of the lizard genus *Phyllodactylus* in North and Central America"; $375.

52. Julian P. Donahue, Michigan State University. "Taxonomy of Indian butterflies: their discrimination, distribution, variation and abundance"; $425.

53. Garnett M. Dow, University of Illinois. "Structure, petrology, and stratigraphy of volcanic and sedimentary sequence of coastal Maine"; $250.

54. Daniel E. Dupree, Northeast Louisiana State College. "An extension of the Cauchy rational function interpolation formula"; $600.

55. Thomas Erber, Illinois Institute of Technology. "Dynamics of a magnetic cooperative system"; $900.

56. Steven E. Farkas, University of New Mexico. "Tertiary volcanic and structural geology of the San Mateo Mountains of west-central New Mexico"; $575.

57. Norma D. Feshbach, University

of Colorado. "The effects of ordinal position on personality as a function of social class"; $600.

58. Klaus H. Fischer, Duke University. "An analysis of the orientation abilities of newly-hatched green turtles, *Chelonia myclas*"; $600.

59. Harold D. Fishbein, Indiana University. "The interaction of classical eyelid conditioning and operant conditioning of the eyelid response"; $400.

60. Maureen P. Fitzgerald, Marymount College, New York, N.Y. "Children's perception of the parental role—its relation to the learning of the sex role and preference for own sex role"; $300.

61. Myrna W. Fivel, University of Pennsylvania. "The development of the concept of numerical equivalence in normal and mentally retarded children"; $625.

62. Sister M. Rita Flaherty, Mount Mercy College, Pittsburgh. "Personality traits of college leaders"; $200.

63. Sister Mary Albert Fleming, Marygrove College, Detroit. "The preparation and coordination chemistry of a variety of compounds containing adjacent nitrogen and phosphorus atoms"; $500.

64. Benjamin A. Foote, Kent State University. "Biology and immature stages of northwestern *Tetanocera* (Diptera: Sciomyzidae)"; $300.

65. Warren C. Forbes, Brown University. "Synthesis and stability of Grunerite"; $700.

66. Robert L. Foster, University of Missouri. "Petrology and geochemistry of some igneous and metamorphic rocks in north-central Alaska"; $500.

67. Sister Alice M. Fox, Clarke College, Dubuque, Iowa. "Mechanisms of intestinal absorption of D-glucose: a study of P/O ratios in the presence of inhibitors"; $300. HSIEN WU & DAISY YEN WU AWARD.

68. David S. Fullerton, Yale University. "Glacial geology of the upper Mohawk region, New York"; $200.

69. Jagannath Ganguly, Indian Institute of Science, Bangalore, India. "Lipid metabolism"; $1200.

70. Earl W. Gardner, Texas Christian University. "Application of indirect fluorescent antibody techniques to the detection of *Vibrio cholerae* antibodies in rabbit serum"; $750.

71. Ram Gopal, Lucknow University, Lucknow, India. "Study of the solutions of high dielectric constant"; $250.

72. Robert B. Gordon, Yale University. "Precision celestial navigation"; $800.

73. Clyde E. Goulden, Yale University. "A systematic revision of the cladoceran genera *Moina*, *Ceriodaphnia*, *Simocephalus*, and *Diaphanosoma*"; $600.

74. Leonard Grosso, Long Island University. "Fetal testicular production of testosterone and Δ^4 androstene 3.17 dione"; $1000.

75. Florence A. Grosvenor, University of Rochester. "Brachiopods in the Rosendale Quadrangle, in the Rondout formation, southeastern New York"; $100.

76. Walter T. Haenggi, University of Texas. "Structural geology and stratigraphy, Cañon de Navarette, Chihuahua, Mexico"; $500.

77. Joseph G. Hall, San Francisco State College. "Ecology of the Kaibab squirrel"; $400.

78. Howard D. Hammond, Howard University. "Systematic investigations of the solanaceae by paper chromatography of phenolic substances"; $200.

79. Larry J. Harms, University of Kansas. "Cytotaxonomic investigations of the genus *Eleocharis* in the United States"; $500.

80. Charles W. Harper, Jr., California Institute of Technology. "Evolution of the brachiopod family *Stropheodontidae* Caster 1939"; $250.

81. Roger A. Hart, Yale University. "The application of oxygen isotopes to economic geology"; $200.

82. William E. Hatfield, University of North Carolina. "The magnetic and spectral properties of vanadyl complexes with tridentate Schiff bases"; $625.

83. Robert P. Higgins, Wake Forest College. "Redescription of *Echinoderes steineri* Chitwood, 1951 from the Texas Gulf Coast and investigation of other regional Kinorhynch fauna"; $325.

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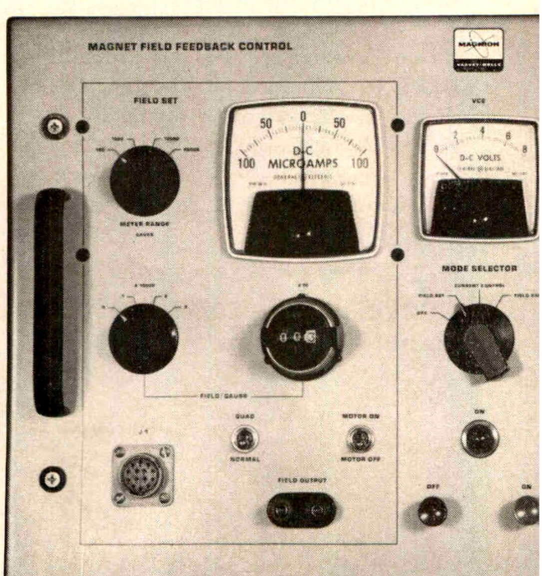


One of a series briefly describing GM's research in depth

THE CURIOUS CRYSTALLOGRAPHY OF FATIGUE

Ever bend a paper clip back and forth till it breaks? That's metal fatigue, a problem that's just beginning to be understood at the atomic level. Unfortunately, there is still no generally accepted explanation as to why repeated loading leads to fatigue cracks and eventual failure.

By stressing copper single crystals in cyclic torsion, physicists at the General Motors Research Laboratories have been able to relate fissure development to crystal orientation—and the type of surface deformation to the amplitude of cyclic strain.

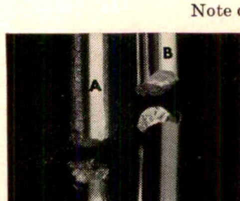
In developing a theory of fatigue, they have found that a fundamental distinction must be made between cycling at high and low strain amplitudes. At high amplitudes the crystal fractures in an irregular manner. But at low amplitudes, the fracture roughly follows the crystal's slip planes.

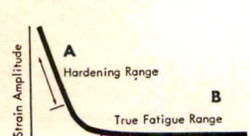
Such basic research may eventually make it possible to predict the fatigue properties of an alloy from a knowledge of its microstructure. It's another example of the "research in depth" approach used by General Motors scientists and engineers to make things better.

General Motors Research Laboratories

Warren, Michigan

Note differences in two fractured single crystals of copper—identically oriented but fatigued at different amplitudes.





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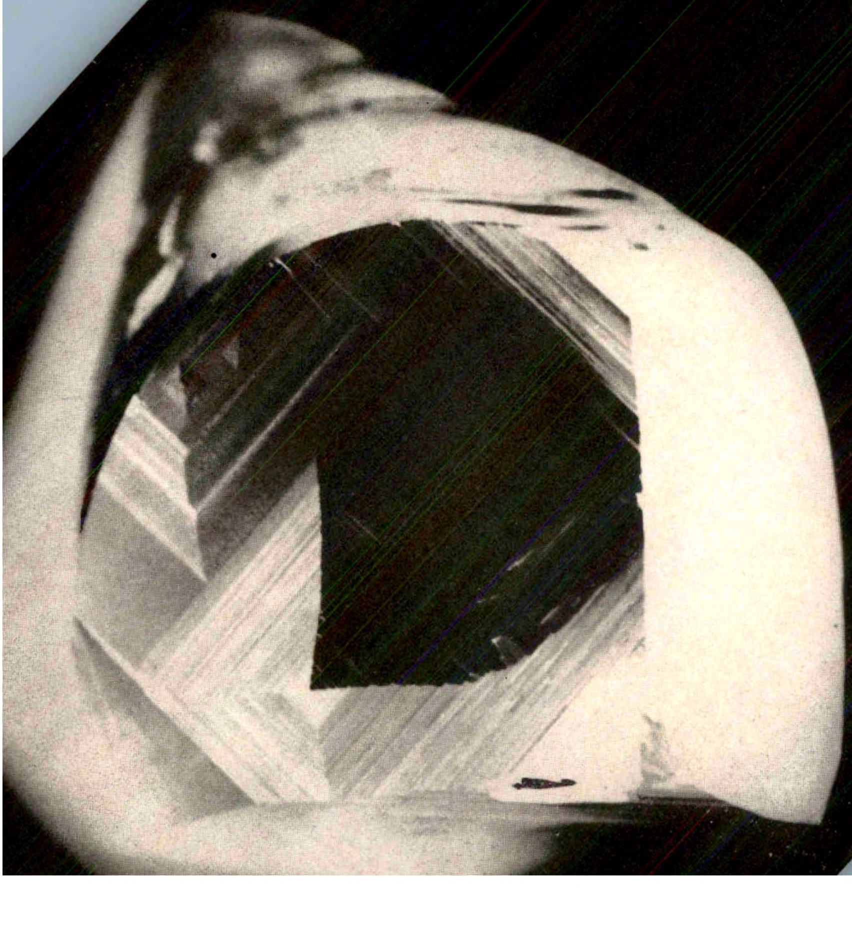

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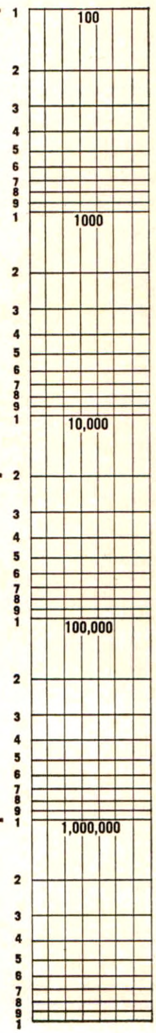

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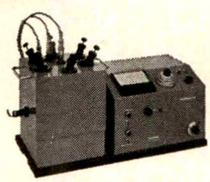

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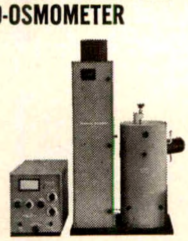

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SCIENCE IS EVERYBODY'S BUSINESS

By J. TUZO WILSON

Throughout most of its brief history science has been regarded as a mystery practiced and understood by few. Its devotees have deliberately shunned the headlines, while engaged in the work which unexpectedly has become the news of the century, for it has altered the world's economy and forever changed relations among countries. Almost overnight, people and governments have awakened to the power of science which in our time has created unprecedented wealth, unconquerable military power, and instant communications for some of the world and hope of these for the rest. Such profound changes have redirected the course of human society. They have affected us all. Suddenly science is everybody's business.

The Power of Science

Many resent this escape of science from the laboratory and its intrusion into their affairs. They do not understand it; they instinctively fear its power; and they dislike its cold and impersonal nature.

Such reactionaries denigrate the effects of science. They say correctly that its impact on society is no more profound than was the discovery of agriculture, that its influences are not so powerful as have been those of the great religions and philosophies of the past. This debate is irrelevant. The strength of the impact of science on us derives from the speed with which it is effecting changes and from the accuracy with which it can be precisely aimed at selected targets. In comparison, the great changes of history have been slow and haphazard. Agriculture took millenia to spread, the great religions centuries, but the effects of science have been cataclysmic in our generation. Its revolutionary effects have burst upon us with a suddenness only to be compared with Genghis Khan's conquest of Asia, the march of the Black Death across the civilized world, and the discovery of the Americas by Columbus. Our whole framework of thought has been shaken and governments are now awakening to the problems posed by the powers of science and the necessity and expense of maintaining it.

Many realize the speed of change, but few appreciate that, unlike social forces of the past, science can be directed by men to specific, chosen aims. In 1940, the idea of releasing atomic power was conceived. Five years and a few billion dollars later that power was generated. In 1951, the International Geophysical Year was proposed, and a start to space travel was incorporated as one of its objectives. Once again, a few years of adequately supported effort produced an astonishing achievement.

The unique power of science is that by it man now has the capacity to transform the material world if he can conceive and plan and organize what he wishes to do. The conquest of famine, the control of disease, the spread of education and the provision of plenty are materially possible. What is lacking is the human capacity to cooperate and plan.

This is what has distinguished the scientific revolution from technological changes of the past.

Until this century, improvements were made by trial and error. Even such recent inventions as the steam engine, the automobile, agricultural and factory machines, the telephone and the motion-picture camera were so developed. Only in our time has the scientific method supplanted trial and error and accelerated technological change in an abrupt and revolutionary way. Being now accustomed to this headlong rush, we accept it as normal, but that is far from the case.

Such speed is unique in history and it seems likely that it cannot last. We are living in a brief and unique period when speed of change has made society malleable and when science has given us power to plan and improve it.

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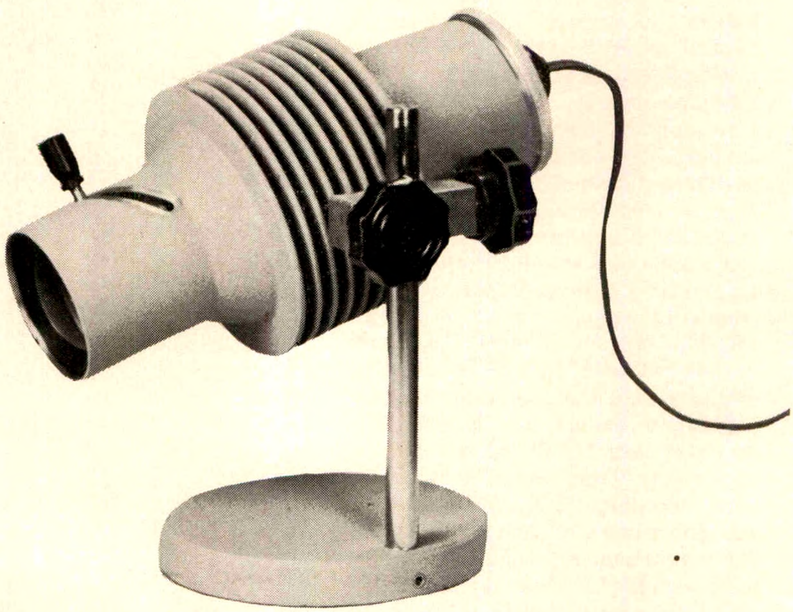

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Our problem is greater than that given to any previous generation. The challenge and the task for today and for tomorrow is to grasp this opportunity for human welfare. Can we rise to it?

How Did Science Become So Important?

To understand the rise of science I think it well to outline the main steps in the development of human skills. There have been three major stages; those in which man learned to control himself, to organize society, and to harness nature. It is a fair approximation to call these three skills the arts, the humanities, and the sciences. These, I suggest, are not three cultures, but are elements which have been combined in additive fashion to create cultures. Of these complex cultures I think that there have been three.

The first was the culture of the primitive savages who understood the arts alone. In the broadest sense, the arts are the skills of the individual: speech, the use of weapons and tools, painting, music, and the dance. A culture depending only on the arts was the culture of primitive man before the discovery of agriculture. These savages lived as nomads in small family groups, hunting and gathering for a living. Their life was like that of animal primates except that self-discipline and a knowledge of the arts distinguished man from the beasts. The Eskimos, some South American Indians, and the bushmen of Australia and South West Africa have been still living in this culture during this century.

The discovery of agriculture ensured an ample food supply and introduced a second culture. Men were then able to settle in villages and develop communities. To organize and control these communities, man invented the humanities: reading, writing, accounting, the skills of law and government. Great religions bloomed and standing armies were mobilized. In this sense the humanities includes the social sciences. But, in acquiring these new skills man did not discard the old ones of his primitive culture. He retained the arts. For example, the ancient civilizations of Greece, Rome, and China were based upon a culture of arts plus humanities. They became so welded together that, in today's universities, faculties of humanities are known as Faculties of Arts. Today's examples of this culture are the have-not nations such as India, China, and most of Africa. That they lack food and the basic necessities of life is because they have not introduced modern technology and science into their economies.

The third culture began when this was done. Now we are in this stage for we have learned how to harness nature, but industrial civilizations and modern nations are not built on science alone. They depend upon a combination of the arts, the humanities, and the sciences. Man cannot attempt to control nature unless he has control of society and of himself. One cannot do chemistry in a jungle, but only in a community of disciplined individuals.

The three elements have thus been combined to form a primitive culture based upon the individual skills or the arts alone, a middle culture based upon organized agricultural societies involving a knowledge of both arts and humanities, and a modern culture of industrialized societies requiring knowledge of all of the arts, humanities, and sciences.

The idea that mankind has evolved through three cultures is not entirely new. Last month[1] C. F. Hackett and R. Asher described "The Human Revolution" by which man evolved from animals. They referred in their paper to two other revolutions described by V. Gordon Childe as the Neolithic and Urban revolutions. I believe that we can say that the three cultures were started by these three revolutions.

Only the developed and industrialized nations have yet reached the third culture. The under-developed countries are in the second culture and a few outlying people are still in the first. Once people advance to a higher stage, they never go back. The reason is easy to understand.

It has been estimated that, in Britain, before the Roman conquest and before the agricultural Bronze Age, no more than a few thousand people could

[1] *Am. Sci.*, *52* (*1*), 70, March 1964.

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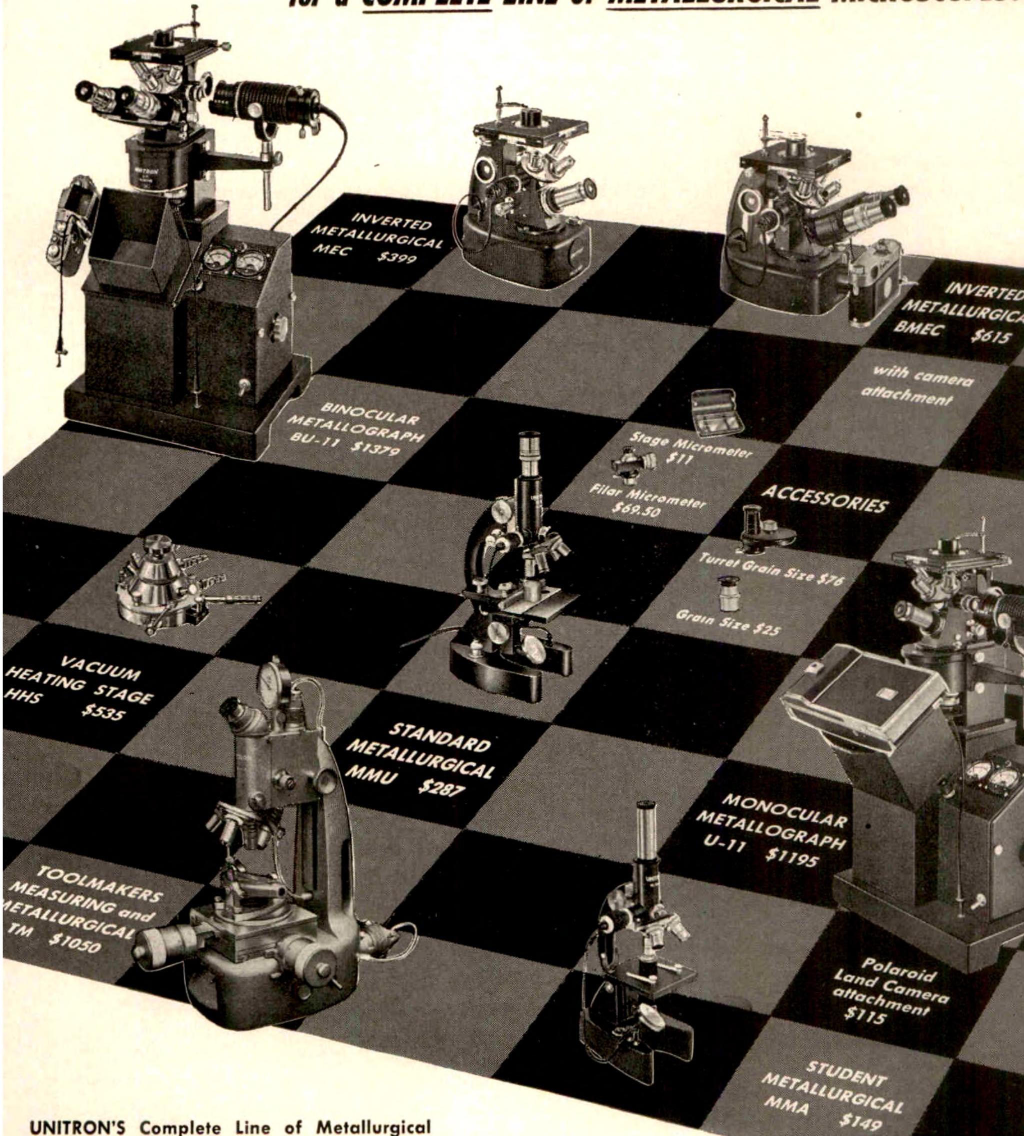

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scrounge a living from that chilly and forested island by hunting and gathering. For millennia that was its total population. When land had been cleared and agriculture introduced, as many as several hundred thousand people lived there during the later Roman Empire and the middle ages. Since the industrial revolution Britain has supported fifty millions, and each step has brought greater, not less, wealth to every man. These numbers mean that, where one savage roamed, a hundred Romans plowed and today ten thousand people flourish. Each culture had roughly a hundred times the population of the last and even greater increases in wealth.

The later stages of the third revolution have been the most dramatic. During this century mechanical technology has been overtaken by more powerful science.

The Effects of Science

The effect of the scientific revolution has differed from country to country.

It has made the United States the richest and most powerful nation the world has ever known. It has brought affluence to three-quarters of the population. It has led to a universal desire for college education. It is redistributing population and wealth within the United States as people flock to centers of new industries. On the other hand, the success of science has failed to give Americans any idea of the limitations of the scientific method. It has given them an exaggerated view of their powers and produced a national trauma when American complacency allowed the Soviets to launch the first artificial satellite. As Adlai Stevenson and Senator Fulbright have just been pointing out, it has also failed to persuade Americans that no reasonable method is open to them to remove communists from power in Cuba and China.

In Canada, the automobile and television have revealed to French Canadians that they have allowed science, modern education, wealth, and power to pass them by. They are reappraising their philosophy, revising their educational system, and demanding a better place for themselves.

Canada's role as a military power has been completely altered. The nuclear deterrent which science has made possible is so much more powerful than other weapons that Canada would not have an important role in a major war. On the other hand, in keeping peace, a new role of great value has been created for middle powers. They are called upon to police troubled areas. Canadian forces have been doing this in Sinai, Yemen, Lebanon, Kashmir, Laos, the Congo, and in Cyprus. Recognizing that deployment of these police forces requires quick and single command, Canada has just embarked on a program to unify all three services under the orders of a single Chief of Defence Staff. It has been announced that it is intended to dress the army, navy, and air force in a common uniform and to have a single line of command and common auxiliary services. This drasticd eparture from tradition is due to the effects of science. Nuclear war has weakened the power of the generals because it requires fewer combatants and would be so disastrous that civilian authorities in every country have acted to curb and alter military activities.

In the United Kingdom the loss of an empire, the miseries of the worst winter in three-hundred years, and failure to enter the Common Market are having the same effect on the national psyche as would an unfortunate love affair on a vigorous woman whose children had grown up and left her. Both political parties maintain that greater support for education and research are the best hope for recovery, and a major theme for the coming British election is how best to implement these improvements. The support of science has become a major political controversy for the first time.

The materialist philosophy of the Soviet Union and of all communist countries gives them faith in science. This faith had its reward in Sputnik I, but exultation turned to bitterness with the realization that planning is fallible, that people need incentives. In Russia, there is a dawning realization that science is good, but incomplete as a guide to life, in fact that Marxist-Leninism is not a workable philosophy.

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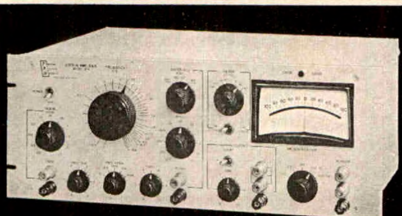

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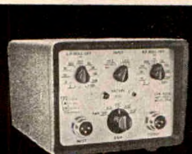

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The developing countries also have faith in science. They are handicapped by vast populations, hunger, poverty, illiteracy, and undeveloped resources. They see that, to achieve the wealth and power of richer countries, they must somehow educate their people and apply scientific procedures to the development of their lands.

To ourselves, and even to an industrialized communist country like Russia, the question of what is a workable and correct philosophy is a matter of great importance; but, to a people on the edge of death by starvation or malnutrition, the choice between different philosophies seems secondary. I think that this can explain the puzzling ambivalence of so many poor and neutralist countries. Hungry and illiterate people are more interested in food and education than they are in democracy.

The Virtues of Science

Three attributes of science which anyone will admit are advantages are that its conclusions are based on experiment and thus reduce argument, that its results are cumulative and ever-increasing, and that any reasonably intelligent and interested person can contribute to its progress. Many great contributions have been made by men of quite ordinary capacity, so that science is a democratic pursuit. Scientists are humble when compared with the leaders in other skills. Indeed, it is held against them that they are dull. Who would compare a chemist with a general, a physicist with a high priest, or a botanist with a prima donna?

One can go farther and suggest that, just as each of the three cultures supported a population a hundred-fold greater than the previous one in vastly greater wealth, so also this change in culture with its increase in prosperity has altered the forms of government. There are forms of government appropriate to each culture.

The primitive savages were truly egalitarian, for none of them had anything. The undeveloped countries and ancient civilizations were a little better off. There was enough for a few to be wealthy, but most were slaves. Only in the developed countries is there wealth enough for everyone to be well-to-do, for everyone to be equal. I believe that it is a fallacy to suppose that poor countries can be democracies. Democracy is made possible by wealth. It is not true that wealth is a consequence of democracy and it is wishful thinking to suppose that democracy can flourish in a state of poverty.

Problems of Science

Most of the problems of science are really problems of dealing with scientists. Be it said to their credit that they are a hard-working, docile, and reasonably intelligent segment of the population, but this is offset by their naïveté and lack of interest in political matters and their aloof attitude to other people and to the problems of society.

Since scientists are ordinary people, who differ only in having been subjected to special training, one cannot say that they have any qualities absolutely different from other people, but some tendencies can be noticed.

Scientists flock like starlings. Nothing is more attractive to scientists than other scientists. They know that new ideas grow fastest in places where ideas are exchanged and challenged. They do not like to go to regions where they are needed because of a dearth of science. They prefer to congregate in the great intellectual centers.

Scientists enjoy an absorbed interest in their work, which they regard as a fascinating puzzle. Achievement is usually less exciting to them than the quest itself. They do not work at science with any humanitarian objectives. They are not concerned with consequences. They are only fascinated to learn how nature operates and pleased to pit their wits in the contest to learn this.

Science is expensive. The cost of educating and equipping scientists is great. Governments tend to expect that grateful scientists should therefore labor to achieve practical and rewarding results, but this is not the aim of most scientists. The less practical their work the better they tend to like it.

They are less influenced by dogma than are other humans. They shun politics with its artificial and unending

personal bickering. A recent survey showed that not a single scientist sat in any major legislative body in Canada and I think this is typical of other countries. They are not good mixers or strong patriots. They are iconoclasts.

They are trained to test things and to be guided by experimental results rather than popular jingo. As a result, they are less ardent patriots and more internationally minded than are others.

Science thus poses a fine problem for governments, which are made up of non-scientists. Science asks support from national governments, while at the same time its total effect is to question and weaken the authority of governments. Science being based on experimental results raises questions about the validity of local prejudices.

For similar reasons, scientists while they may be interested in logic and enjoy good literature have not much use for metaphysics or for scholarly criticism since these are based upon unprovable premises.

Scientists form a very exclusive elite, who look down on all attempts by non-scientists to interfere with or control scientific matters, a rather ridiculous attitude since most scientists know little of any branch of science other than their own. What is more, any intelligent person is able to master a broad view of what science is about, but their views are not accepted as valid by scientists and thus amateur enthusiasm withers in the cold indifferences of the professional.

Scientists have a clearly conceived pecking order, with the most practical scientists at the bottom, and they usually have a poor opinion of research as practiced in other subjects.

These characteristics pose problems even for scientists themselves. When they were few and their demands small, their anti-social ways were overlooked. Now, in these days of big science, they are being examined critically.

In the early days of science, academies were founded in each country to support the experimental method and to provide forums for discussion of results. Government support has now far outstripped the help possible from academies and science has become so complex that many specialized associations are needed for separate discussions. The academies feel left behind and in some countries they are now holding discussions about what their future role should be.

The Future of Science

I have tried to point out the present importance of science, how it was achieved and some of the problems the future poses. Science is clearly indispensable to society, but it is not socially inclined.

Jacques Barzun has recently turned his critical attention upon science, which he has labeled "The glorious entertainment." Many of his criticisms are just, but I think one could as justly call any intellectual pursuits "entertainment." I am not clear why that description might not equally be applied to the study of philosophy, history, or literary criticism. Barzun summarizes his conclusions thus:

> It is not my aim, even if it were within my capacity, to furnish a philosophy. My purpose remains to turn over the pieces of our scientific culture and point out their workings, which by common report are bedeviling the world. Accordingly, I reach the end of my description with the conviction I announced at the beginning, that until western man reasserts his right to be, as far as he can, a natural and moral philosopher, he will feel like an exile in his own place."

The notion that science is bedeviling the world seems to me to reflect a medieval attitude. Even if it is correct, we cannot go back. Without modern technology, present populations could not be supported. Most people would die. It is as simple as that and I feel that it would be as valid therefore to ask all philosophers to become scientists as is Barzun's demand that all scientists become philosophers.

There is confusion here I think, and it lies in Barzun's transposing the needs of a society to each individual. I grant that our society needs a philosophy and perhaps that is all he means, but I think it unreasonable to demand that everyone in it be a philosopher.

Our society depends upon much more than any individual can encompass. It depends on all the arts, humanities, and sciences, but no one person can be artist, humanist, and scientist all in one. Whose role is it then to decide upon our national policies?

I don't think that it is a role for scientists. They do not want to do it and I have indicated why I think they would do it badly. After all, physics is the study of mass and energy. The results of these studies have affected human affairs, but only indirectly. Before a physicist can interest himself in human affairs he must stop being a physicist for the duration of that concern.

The same would be true for an artist. The only proper person to concern himself with the problems of society is the humanist, but he must learn to concern himself with society in a scientific age and not with society in an age that is past. I suggest it is for philosophers to study the effects of science not for physicists to seek a philosophy.

The introduction of agriculture and community life altered the role of the artist. In primitive societies artists were free. They expressed their feelings in wild and imaginative ways, which is the appeal of Eskimo art. In civilized societies, the artist was tamed and harnessed to the needs of society. The old masters were not free to follow their imagination in painting. Instead they followed the wishes of the rulers of church and state. They have only been freed of restraints by the invention of photography.

Today it is the humanist's turn to be harnessed to a more complex society. The demands placed upon humanists have been altered by the advent of science. Have they responded?

Snow maintains that scientists and humanists don't speak to one another, but the reasons are different. Most scientists, as private individuals, know something of the humanities; the problem is that they don't care about the problems of society. Most humanists on the other hand care very much and, like Barzun, worry about these problems. Their difficulty is that most of them have not yet caught up with the twentieth century and do not know enough about science to take its impact into proper account.

Some economists in particular will deny this, and I admit that there are distinguished exceptions; but I think the fact that we are governed and that we derive most of our social and business guidance from illiterates in science is a sufficiently serious problem to demand attention from universities.

I certainly don't recommend stopping the study of any branch of learning, nor do I think that old departments can easily be deflected into new directions. I don't think it is any more likely that old philosophers will learn new science than that scientists will willingly study philosophy.

To meet a new requirement, I suggest that universities form new Departments of Scientific Studies directed to the training of scientifically literate humanists.

How is this to be done? There is a danger that people who are taught a potpourri of science, of history, of economics, of philosophy, and so forth, would be poorly trained in all. I believe that the solution is to base such studies on a thorough reading of the history of science starting from the earliest times.

The lectures should be accompanied and illustrated throughout the course by laboratory experiments repeating some of the great scientific discoveries in their historic order. These should be executed with the same rigor as is required in the various science departments, but they should be less numerous, so that the student would have time for reading and essay writing. There should be no attempt to complete the scientific training of these students. In the senior years, they should direct their laboratory work to computing and data processing—techniques now recognized to be as useful in the social as in the pure sciences. It should be clearly kept in mind that they are humanists, but humanists with an experience of experimental work, able to read about and comprehend the effects of science.

There is an additional reason for making this effort to produce a man with

broad knowledge of the humanities and sciences.

There has been a recent change in the main direction of sciences. Hotly as many scientists will deny it, science is as much subject to fashion as other human activities. In the last century, geology and evolution, chemistry and engineering held the center of the stage. For a period early in this one, nuclear physics was paramount. Today, studies of planets and of space are important, but even more so are the group of researches directed to understanding the brain, including information and computer theory, genetics and the DNA code and biological brain research.

This is a drastic change in the direction of science. Studies of engineering, chemistry, and physics all concerned the study of matter and energy which are extensions of man's muscles. Brain research and the like are directed to extensions of man's mind. But surely, man's mind is a proper study for humanists. Thus, the forefront of scientific research has now entered upon one of the chosen fields of the humanities. I feel it is appropriate to start departments directed to study of the humanities in the light of modern science.

THE EXECUTIVE SECRETARY'S PAGE

The Sixty-fourth Annual Convention of The Society of the Sigma Xi on December 30, 1963, amended the National Constitution and Bylaws to give to the Membership-at-Large chapter status with the designation Chapter-at-Large. These actions also empowered this group to elect new Members in accordance with the provision of Article VII, Section 2(c) of the National Constitution, and to promote from associate membership in accordance with Article VIII, Section 2 of the National Constitution.

By Article VIII, Section 1(e), the Committee on Membership-at-Large is charged with the additional responsibility of serving as the Committee on Admissions for the Chapter-at-Large.

Procedures for initiating and continuing the fulfilling of these obligations have been formulated by the Committee on Membership-at-Large and approved by the National Executive Committee.

ELECTION AND PROMOTION OF INDIVIDUALS TO FULL MEMBERSHIP IN THE SOCIETY OF THE SIGMA XI BY THE CHAPTER-AT-LARGE

A. **Election:**

I. *Basis:* The policy of the Society for such action by the Chapter-at-Large is based on the knowledge that there are some persons who have attained recognition of their accomplishment in scientific research but who have been missed in the established procedures of the Society, for example:

1. Academic work was done at an institition where there was no chapter.
2. Scientific research accomplishments occurred after completion of academic program.
3. In exceptional cases, significant research accomplishments may have been achieved without the benefit of a formal advanced scientific education.
4. Eligibility overlooked by the appropriate institutional chapter Committee on Admissions.

II. *Criteria:*

1. A person who is a member of the staff or a student of an institution having a chapter is *not* eligible for election by this procedure.
2. The basic criteria for election will be such accomplishments as would justify election by an institutional chapter.

III. *Procedure:*

1. As the Society's procedures do not permit application for membership, a person before election must be proposed (nominated) by two active Members of the Society.
   a. Nomination is to be made upon standard *Nomination for Membership* form obtainable from Executive Secretary.
   b. Information called for should be complete.
2. Executive Secretary will prepare and send copies of nominations properly completed to all members of the Committee on Membership-at-Large.
3. Members of Committee on Membership-at-Large will indicate their approval or raise questions by mail ballot.
4. If approval is unanimous the nomination will go before next meeting of the Executive Committee in accordance with Article VII, Section 2(c) of the Constitution.
5. If not unanimous the nomination will be discussed at next meeting of the Committee on Admissions for the Chapter-at-Large.
6. The Executive Secretary will notify the nominee and nominators of the action of the Executive Committee. (In the case of referral to the chapter its usual procedure will be followed.) In case the nomination is not considered favorably, the nominators only will be notified.

B. **Promotion:**

I. *Basis:* The general basis for the promotion of Associate Members to (Full) membership in the Society will be:

1. The research work of a nominee done after his election to associate membership is of such a quality and amount that he is considered eligible for election to (Full) membership, even if the nominee is not now actively pursuing independent research work.
2. The nominee has been and now is engaged in research work of a quality and extent that is considered by the Committee to justify his election to (Full) membership.

II. *Criteria:*

1. Associate Members who are either staff members or students at institutions having chapters, and Associate Members actually affiliated with institutional chapters, will *not* be processed for promotion by the Committee on Admissions for the Chapter-at-Large.
2. The basic criteria for promotion will be such accomplishment as would justify promotion by an institutional chapter.

III. *Procedure:*

1. An Associate Member before promotion must be proposed (nominated) for this advancement by two active Members of the Society.
   a. Nomination is to be made upon standard *Nomination for Membership* form obtainable from Executive Secretary.
   b. Information called for should be complete and nominators should indicate on form basis upon

which nominee was originally elected to associate membership as well as subsequent data in support of promotion.

2. Executive Secretary will prepare and send copies of nominations properly completed to all members of the Committee on Membership-at-Large.
3. Members of Committee on Membership-at-Large will indicate their approval or raise questions by mail ballot.
4. Nominations receiving notes of disapproval will be reviewed in meetings of the Committee.
5. If a nomination is not considered favorably, the Executive Secretary will notify the nominators, when this can appropriately be done, of the reasons why the nomination was not considered favorably.
6. In the case of a promotion approved by the Committee, the Executive Secretary will notify the nominee, with copies to the nominators.

C. **Initiation:**

Initiation shall be in accordance with Article IX, Section 2(b) of the Constitution.

D. **Public Announcement:**

1. The *American Scientist* will be requested to report the election and promotion of Members by this procedure.
2. A press release will be made to proper media in each case of election and promotion by these procedures.

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BOOKS RECEIVED FOR REVIEW

TO THE MEMBERSHIP: *Our readers are reminded that this section of their journal is intended to be an information service listing first editions of the most important new scientific hardbound and paperbound books received for review in our Princeton Editorial Offices. Titles are for late 1963 and early 1964 publications unless otherwise noted.*

From **Academic Press:**

Three 1963 volumes from Italian Physical Society, Proceedings of the International School of Physics, "Enrico Fermi":

Course 19, *Cosmic Rays, Solar Particles, & Space Research,* edited by B. PETERS; 418 pages; $16.

Course 21, *Liquid Helium,* edited by G. CARERI; 442 pages; $16.

Course 22, *Semi-Conductors,* edited by R. A. SMITH; 540 pages; $22.

Mineral Metabolism, An Advanced Treatise, Vol. 2, Part A: The Elements, edited by C. L. COMAR & F. BRONNER; 649 pages; $22.

Primitive Motile Systems in Cell Biology, edited by R. D. ALLEN & N. KAMIYA; 642 pages; $22.

Physiological Mammalogy, Vol. I: Mammalian Populations, edited by W. MAYER & R. VAN GELDER; 381 pages; $12.

Radiation, Isotopes, & Bone by F. C. MCLEAN & A. M. BUDY; 216 pages; $5.95 cloth; $3.45 paper.

The Cell: Biochemistry, Physiology, Morphology, Vol. VI, edited by J. BRACHET & A. MIRSKY; 564 pages; $18 to nonsubscribers.

International Review of Cytology, Vol. 16, edited by G. H. BOURNE & J. F. DANIELLI; 345 pages; $14.

Mammalian Protein Metabolism, Vol. I, edited by H. N. MUNRO & J. B. ALLISON; 566 pages; $18.50.

Physiology of Mollusca, Vol. I, edited by K. M. WILBUR & M. YONGE; 473 pages; $16.

Cell Differentiation, edited by G. E. FOGG; 404 pages; $14. Vol. 17 of *Symposia of the Society for Experimental Biology.*

Residue Reviews, Vol. V: Instrumentation for the Detection & Determination of Pesticides & Other Residues in Foods (Symposium of Los Angeles Meetings, American Chemical Society, April 1963), edited by F. A. GUNTHER; 176 pages; $6.50.

Methods of Experimental Physics, Vol. II: Electronic Methods, edited by E. BLEULER & R. O. HAXBY; 839 pages; $24.

Elastic Liquids (An Introductory Vector Treatment of Finite-strain Polymer Rheology) by A. S. LODGE; 389 pages; $12, London.

Chelating Agents & Metal Chelates, edited by F. P. DWYER & D. P. MELLOR; 530 pages; $17.

The Physiological Clock, Endogenous Diurnal Rhythms & Biological Chronometry by E. BUNNING; 145 pages; $6.50.

Physical Acoustics, Principles & Methods, Vol. I, Part A: Methods & Devices, edited by W. P. MASON; 515 pages; $17.

Fundamentals of Heat Transfer by S. S. KUTATELADZE; 485 pages; $14.50.

Infrared Absorption Spectra, Index for 1958–1962 by H. M. HERSHENSON; 153 pages; $12.

Comparative Nutrition of Man & Domestic Animals, Vol. 2, by H. H. MITCHELL; 840 pages; no price given.

Synchytrium by J. S. KARLING; 470 pages; $17.50.

Molecular Pharmacology, The Mode of Action of Biologically Active Compounds, Vol. I, edited by E. J. ARIËNS; 503 pages; $17.

Theory of Excitons by R. S. KNOX; 207 pages; $8.50.

Categories of Human Learning, edited by A. W. MELTON; 356 pages; $8.50.

Modern Developments in Electron Microscopy, edited by B. M. SIEGEL; 432 pages; $13.50.

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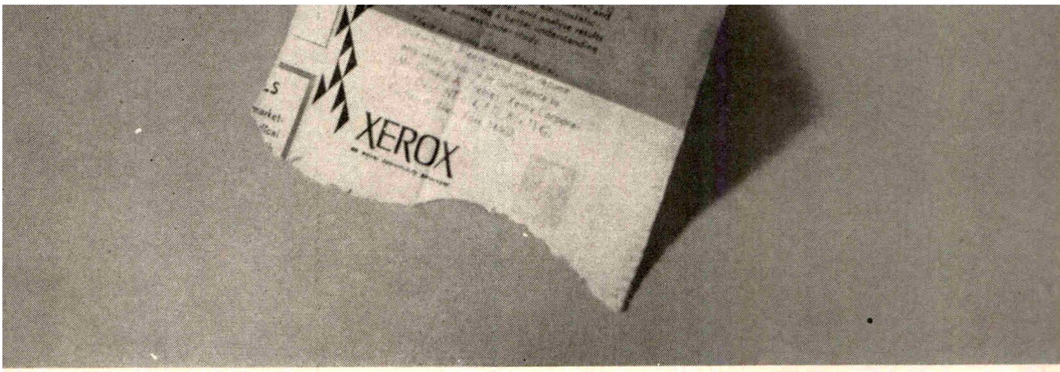

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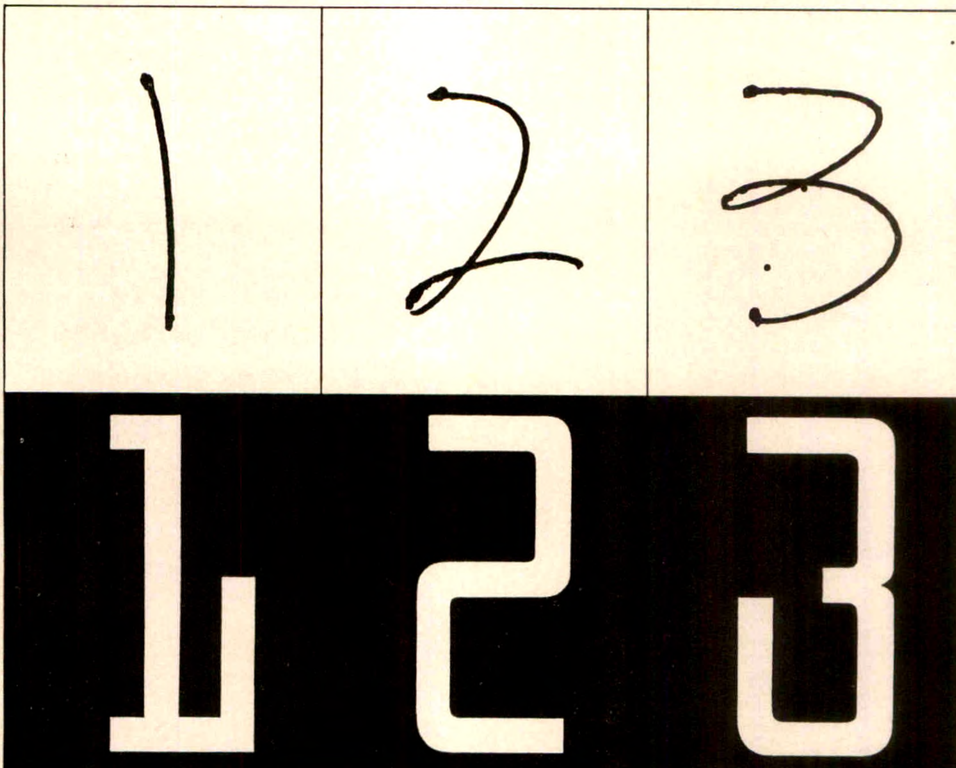

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HARVEY A. NEVILLE

The ninth President of Lehigh University, serving in that capacity since 1960, and associated with Lehigh since 1927, Dr. Neville was elected, at the Sixty-fourth Annual Convention in Cleveland, Ohio, in December 1963, to a five-year term as Treasurer of the Society of the Sigma Xi. He succeeded Donald Bishop Prentice. A chemist by profession, he gained an A.B. from Randolph-Macon College and Ph.D. from Princeton University. The recipient of the Hillman Award of the Lehigh Faculty in 1948 and of honorary degrees from Randolph-Macon in 1952, and from Moravian and Lafayette Colleges in 1962, he becomes President Emeritus of Lehigh at the Founder's Day Exercises, October 11, 1964.

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INFORMATION PROCESSING IN COMPUTER AND MAN*

By HERBERT A. SIMON and ALLEN NEWELL

ORGANIZING a computer to perform complex tasks depends very much more upon the characteristics of the task environment than upon the "hardware"—the specific physical means for realizing the processing in the computer. Thus, all past and present digital computers perform basically the same kinds of symbol manipulations.

In programing a computer it is substantially irrelevant what physical processes and devices—electromagnetic, electronic, or what not—accomplish the manipulations. A program, written in one of the symbolic programing languages, like ALGOL or FORTRAN, will produce the same symbolic output on a machine that uses electron tubes for processing and storing symbols, one that incorporates magnetic drums, one with a magnetic core memory, or one with completely transistorized circuitry. The program, the organization of symbol-manipulating processes, is what determines the transformation of input into output. In fact, provided with only the program output, and without information about the processing speed, one cannot determine what kinds of physical devices accomplished the transformations: whether the program was executed by a solid-state computer, an electron-tube device, an electrical relay machine, or a room full of statistical clerks! Only the organization of the processes is determinate. Out of this observation arises the possibility of an independent science of information processing.

By the same token, since the thinking human being is also an information processor, it should be possible to study his processes and their organization independently of the details of the biological mechanisms—the "hardware"—that implement them. The output of the processes, the behavior of *Homo cogitans,* should reveal how the information processing is organized, without necessarily providing much information

* A Sigma Xi-RESA National Lecture delivered by Dr. Herbert A. Simon, 1963–64.

about the protoplasmic structures or biochemical processes that implement it. From this observation follows the possibility of constructing and testing psychological theories to explain human thinking in terms of the organization of information processes; and of accomplishing this without waiting until the neurophysiological foundations at the next lower level of explanation have been constructed.

Finally, there is a growing body of evidence that the elementary information processes used by the human brain in thinking are highly similar to a subset of the elementary information processes that are incorporated in the instruction codes of present-day computers. As a consequence it has been found possible to test information-processing theories of human thinking by formulating these theories as computer programs—organizations of the elementary information processes—and examining the outputs of computers so programed. The procedure assumes no similarity between computer and brain at the "hardware" level, only similarity in their capacities for executing and organizing elementary information processes. From this hypothesis has grown up a fruitful collaboration between research in "artificial intelligence," aimed at enlarging the capabilities of computers, and research in human cognitive psychology.

These, then, are the three propositions on which this discussion rests:

(1) A science of information processing can be constructed that is substantially independent of the specific properties of particular information processing mechanisms.

(2) Human thinking can be explained in information-processing terms without waiting for a theory of the underlying neurological mechanisms.

(3) Information-processing theories of human thinking can be formulated in computer programing languages, and can be tested by simulating the predicted behavior with computers.

Levels of Explanation

No apology is needed for carrying explanation only to an intermediate level, leaving further reduction to the future progress of science. The other sciences provide numerous precedents, perhaps the most relevant being nineteenth-century chemistry. The atomic theory and the theory of chemical combination were invented and developed rapidly and fruitfully during the first three-quarters of the nineteenth century—from Dalton, through Kekulé, to Mendeleev—without any direct physical evidence for or description of atoms, molecules, or valences. To quote Pauling: [1]

> Most of the general principles of molecular structure and the nature of the chemical bond were formulated long ago by chemists by induction from the great body of chemical facts. . .

> The study of the structure of molecules was originally carried on by chemists using methods of investigation that were essentially chemical in nature, relating to the chemical composition of substances, the existence of isomers, the nature of the chemical reactions in which a substance takes part, and so on. From the consideration of facts of this kind Frankland, Kekulé, Couper, and Butlerov were led a century ago to formulate the theory of valence and to write the first structural formulas for molecules, van't Hoff and le Bel were led to bring classical organic stereochemistry into its final form by their brilliant postulate of the tetrahedral orientation of the four valence bonds of the carbon atom, and Werner was led to his development of the theory of the stereochemistry of complex inorganic substances.

The history this passage outlines is worth pondering, because the last generation of psychologists has engaged in so much methodological dispute about the nature, utility, and even propriety, of theory. The vocal, methodologically self-conscious, behaviorist wing of experimental psychology has expressed its scepticism of "unobserved entities" and "intermediate constructs" [2]. Sometimes it has seemed to object to filling the thinking head with anything whatsoever. Psychologists who rejected the empty-head viewpoint, but who were sensitive to the demand for operational constructs tended to counter the behaviorist objections by couching their theories in physiological language [3].

The example of atomic theory in chemistry shows that neither horn of this dilemma need be seized. On the one hand, hypothetical entities, postulated because they were powerful and fruitful for organizing experimental evidence, proved exceedingly valuable in that science, and did not produce objectionable metaphysics. Indeed, they were ultimately legitimized in the present century by "direct" physical evidence.

On the other hand, the hypothetical entities of atomic theory initially had no *physical* properties (other than weight) that could explain why they behaved as they did. While an electrical theory of atomic attraction predated valence theory, the former hypothesis actually impeded the development of the latter and had to be discredited before the experimental facts could fall into place. The valence of the mid-century chemist was a "chemical affinity" without any underlying physical mechanism. So it remained for more than half a century until the electron-shell theory was developed by Lewis and others to explain it.

Paralleling this example from chemistry, information-processing theories of human thinking employ unobserved entities—symbols—and unobserved processes—elementary information processes. The theories provide explanations of behavior that are mechanistic without being physiological. That they are mechanistic—that they postulate only processes capable of being effected by mechanism—is guaranteed by simulating the behavior predicted on ordinary digital computers. (See the Appendix, "Computer Programs as Theories.") Simulation provides a basis for testing the predictions of the theories,

but does not imply that the protoplasm in the brain resembles the electronic components of the computer.

A Specific Information-Processing Theory: Problem Solving in Chess

Information-processing theories have been constructed for several kinds of behavior, and undertake to explain behavior in varying degrees of detail. As a first example, we consider a theory that deals with a rather narrow and special range of human problem-solving skill, attempting to explain the macroscopic organization of thought in a particular task environment.

Good chess players often detect strategies—called in chess, "combinations"—that impose a loss of a piece or a checkmate on the opponent over a series of moves, no matter what the latter does in reply. In actual game positions where a checkmating possibility exists, a strong player may spend a quarter hour or more discovering it, and verifying the correctness of his strategy. In doing so, he may have to look ahead four or five moves, or even more [4]. If the combination is deep, weaker players may not be able to discover it at all, even after protracted search. How do good players solve such problems? How do they find combinations?

A theory now exists that answers these questions in some detail. First, I shall describe what it asserts about the processes going on in the mind of the chess player as he studies the position before him, and what it predicts about his progress in discovering an effective strategy. Then we can see to what extent it accounts for the observed facts. The actual theory is a computer program couched in a list-processing language, called Information Processing Language V (IPL-V). Our account of the theory will be an English-language translation of the main features of the program [5].

The statement of the theory has five main parts. The first two of these specify the way in which the chess player stores in memory his representation of the chess position, and his representation of the moves he is considering, respectively. The remaining parts of the theory specify the processes he has available for extracting information from these representations and using that information: processes for discovering relations among the pieces and squares of the chess position, for synthesizing chess moves for consideration, and for organizing his search among alternative move sequences. We shall describe briefly each of these five parts of the theory.

The theory asserts, first of all, that *the human chess player has means for storing internally, in his brain, encoded representations of the stimuli presented to him.* In the case of a highly schematized stimulus like a chess position, the internal symbolic structure representing it can be visualized as similar to the printed diagram used to represent it in a

chess book. The internal representation employs symbols that name the squares and the pieces, and symbolizes the relations among squares, among pieces, and between squares and pieces.

For example, the internal representation symbolizes rather explicitly that a piece on the King's square is a Knight's-move away, in a SSW direction, from a piece on the third rank of the Queen's file. Similarly, if the King's Knight is on the King's Bishop's Third square (KB3), the representation associates the symbol designating the Knight with the symbol designating the KB3 square, and the symbol designating the square with that designating the Knight. On the other hand, the representation does not symbolize directly that two pieces stand on the same diagonal. Relations like this must be discovered or inferred from the representation by the processes to be discussed below.

Asserting that a position is symbolized internally in this way does not mean that the internal representations are verbal (any more than the diagrams in a chess book are verbal). It would be more appropriate, in fact, to describe the representations as a "visual image," provided that this phrase is not taken to imply that the chess player has any conscious explicit image of the entire board in his "mind's eye."

The chess player also has means for representing in memory the moves he is considering. He has symbol-manipulating processes that enable him, from his representations of a position and of a move, to use the latter to modify the former—the symbolic structure that describes the position—into a new structure that represents what the position would be *after* the move. The same processes enable him to "unmake" a move—to symbolize the position as it was before the move was considered. Thus, if the move that transfers the King's Knight from his original square (KN1) to the King's Bishop's Third square (KB3) is stored in memory, the processes in question can alter the representation of the board by changing the name of the square associated with the Knight from KN1 to KB3, and conversely for unmaking the move.

The chess player has processes that enable him to discover new relations in a position, to symbolize these, and to store the information in memory. For example, in a position he is studying (whether the actual one on the board, or one he has produced by considering moves), he can discover whether his King is in check—attacked by an enemy man; or whether a specified piece can move to a specified square; or whether a specified man is defended. The processes for detecting such relations are usually called perceptual processes. They are characterized by the fact that they are relatively direct: they obtain the desired information from the representation with a relatively small amount of manipulation.

The chess player has processes, making use of the perceptual processes, that permit him to generate or synthesize for his consideration moves with specified properties—for example, to generate all moves that will check

the enemy King. To generate moves having desired characteristics may require a considerable amount of processing. If this were not so, if any kind of move could be discovered effortlessly, the entire check-mating program would consist of the single elementary process: DISCOVER CHECKMATING MOVES.

An example of these more complex, indirect processes is a procedure that would discover certain forking moves (moves that attack two pieces simultaneously) somewhat as follows:

Find the square of the opposing Queen. Find all squares that lie a Knight's-move from this square. Determine for each of these squares whether it is defended (whether an opposing piece can move to it). If not, test all squares a Knight's-move away from it to see if any of them has a piece that is undefended or that is more valuable than a Knight.

Finally, the chess player has processes for organizing a search for mating combinations through the "tree" of possible move sequences. This search makes use of the processes already enumerated, and proceeds as follows:

The player generates all the checking moves available to him in the given position, and for each checking move, generates the legal replies open to his opponent. If there are no checking moves, he concludes that no checkmating combination can be discovered in the position, and stops his search. If, for one of the checking moves, he discovers there are no legal replies, he concludes that the checking move in question is a checkmate. If, for one of the checking moves, he discovers that the opponent has more than four replies, he concludes that this checking move is unpromising, and does not explore it further.

Next, the player considers all the checking moves (*a*) that he has not yet explored and (*b*) that he has not yet evaluated as "CHECKMATE" or "NO MATE." He selects the move that is most promising—by criteria to be mentioned presently—and pushes his analysis of that move one move deeper. That is, he considers each of its replies in turn, generates the checking moves available after those replies, and the replies to those checking moves. He applies the criteria of the previous paragraph to attach "CHECKMATE" or "NO MATE" labels to the moves when he can. He also "propagates" these labels to antecedent moves. For example, a reply is labeled CHECKMATE if at least one of its derivative checking moves is CHECKMATE; it is labeled NO MATE if all the consequent checking moves are so labeled. A checking move is labeled CHECKMATE if all of the replies are so labeled; it is labeled NO MATE if at least one reply is so labeled.

The most promising checking move for further exploration is selected by these criteria: that checking move to which there are the fewest replies receives first priority [6]. If two or more checking moves are tied on this criterion, a double check (check with two pieces) is given priority

over a single check. If there is still a tie, a check that does not permit a recapture by the opponent is given priority over one that does. Any remaining ties are resolved by selecting the check generated most recently.

A number of details have been omitted from this description, but it indicates the theory's general structure and the kinds of processes incorporated. The theory predicts, for any chess position that is presented to it, whether a chess player will discover a mating combination in that position, what moves he will consider and explore in his search for the combination, and which combination (if there are several alternatives, as there often are) he will discover. These predictions can be compared directly with published analyses of historical chess positions or tape recordings of the thinking-aloud behavior of human chess players to whom the same position is presented.

Now it is unlikely that, if a chess position were presented to a large number of players, all of them would explore it in exactly the same way. Certainly strong players would behave differently from weak players. Hence, the information-processing theory, if it is a correct theory at all, must be a theory only for players of a certain strength. On the other hand, we would not regard its explanation of chess playing as very satisfactory if we had to construct an entirely new theory for each player we studied.

Matters are not so bad, however. First, the interpersonal variations in search for chess moves in middle-game positions appear to be quite small for players at a common level of strength as we shall see in a moment. Second, some of the differences that are unrelated to playing strength appear to correspond to quite simple variants of the program—altering, for example, the criteria that are used to select the most promising checking move for exploration. Other differences, on the other hand, have major effects on the efficacy of the search, and some of these, also, can be represented quite simply by variants of the program organization. Thus, the basic structure of the program, and the assumptions it incorporates about human information-processing capacities, provides a general explanation for the behavior, while particular variants of this basic program allow specific predictions to be made of the behavioral consequences of individual differences in program organization and content.

The kinds of information the theory provides, and the ways in which it has been tested can be illustrated by a pair of examples. Adriaan de Groot [7] has gathered and analyzed a substantial number of thinking-aloud protocols, some of them from grandmasters. He uniformly finds that, even in complicated positions, a player seldom generates a "tree" of more than fifty or seventy-five positions before he chooses his move. Moreover, the size of the tree does not depend on the player's strength.

The thinking-aloud technique probably underestimates the size of the search tree somewhat, for a player may fail to mention some variations he has seen, but the whole tree is probably not an order of magnitude greater than that reported.

In forty positions from a standard published work on mating combinations where the information-processing theory predicted that a player would find mating strategies, the median size of its search tree ranged from 13 positions for two-move mates, to 53 for five-move mates. A six-move mate was found with a tree of 95 positions; and an eight-move mate with a tree of 108. (The last two mates, as well as a number of the others, were from historically celebrated games between grandmasters, and are among the most "brilliant" on record.) Hence, we can conclude that the predictions of the theory on amount of search are quite consistent with de Groot's empirical findings on the behavior of highly-skilled human chess players.

The second example tests a much more detailed feature of the theory. In the eight-move mate mentioned above, it had been known that by following a different strategy the mate could have been achieved in seven moves. Both the human grandmaster (Edward Lasker in the game of Lasker-Thomas, 1912) and the program found the eight-move mate. Examination of the exploration shows that the shorter sequence could only have been discovered by exploring a branch of the tree that permitted the defender two replies before exploring a branch that permitted a single reply. The historical evidence here confirms the postulate of the theory that players use the "fewest replies" heuristic to guide their search. (The evidence was discovered after the theory was constructed.) A second piece of evidence of the same sort has been found in a recent game between experts reported in *Chess Life* (December 1963). The winner discovered a seven-move mate, but overlooked the fact that he could have mated in three moves. The annotator of the game, a master, also overlooked the shorter sequence. Again, it could only have been found by exploring a check with two replies before exploring one with a single reply.

The "fewest replies" heuristic is not a superficial aspect of the players' search, nor is its relevance limited to the game of chess. Most problem-solving tasks—for example, discovering proofs of mathematical theorems—require a search through a branching "tree" of possibilities. Since the tree branches geometrically, solving a problem of any difficulty would call for a search of completely unmanageable scope (numbers like 10^{120} arise frequently in estimating the magnitude of such searches), if there were not at hand powerful heuristics, or rules of thumb, for selecting the promising branches for exploration. Such heuristics permit the discovery of proofs for theorems (and mating combinations) with the limited explorations reported here.

The "fewest replies" heuristic is powerful because it combines two functions: It points search in those directions that are most restrictive for the opponent, giving him the least opportunity to solve his problem; at the same time, it limits the growth of the search tree, by keeping its rate of branching as low as possible. The "fewest replies" heuristic is the basis for the idea of retaining the initiative in military strategy, and in competitive activities generally, and is also a central heuristic in decision-making in the face of uncertainty. Hence, its appearance in the chess playing theory, and in the behavior of the human players, is not fortuitous.

Parsimonious and Garrulous Theories

Granting its success in predicting both some general and some very specific aspects of human behavior in chess playing, like the examples just described, the theory might be confronted with several kinds of questions and objections. It somehow fails to conform to our usual notions of generality and parsimony in theory. First, it is highly specific—the checkmating theory purports to provide an explanation only of how good chess players behave when they are confronted with a position on the board that calls for a vigorous mating attack. If we were to try to explain the whole range of human behavior, over all the enormous variety of tasks that particular human beings perform, we should have to compound the explanations from thousands of specific theories like the checkmate program. The final product would be an enormous compendium of "recipes" for human behavior at specific levels of skill in specific task environments [8].

Second, the individual theories comprising this compendium would hardly be parsimonious, judged by ordinary standards. We used about a thousand words above to provide an approximate description of the checkmate program. The actual program—the formal theory—consists of about three thousand computer instructions in a list-processing language, equivalent in information content to about the same number of English words. (It should be mentioned that the program includes a complete statement of the rules of chess, so that only a small part of the total is given over to the description of the player's selection rules and their organization.)

Before we recoil from this unwieldy compendium as too unpleasant and unaesthetic to contemplate, let us see how it compares in bulk with theories in the other sciences. With the simplicity of Newtonian mechanics (why is this always the first example to which we turn?), there is, of course, no comparison. If classical mechanics is the model, then a theory should consist of three sentences, or a couple of differential equations [9].

But chemistry, and particularly organic chemistry, presents a dif-

ferent picture. It is perhaps not completely misleading to compare the question "How does a chess player find a checkmating combination?" with a question like "How do photoreceptors in the human eye operate?" or "How is the carbohydrate and oxygen intake of a rabbit transformed into energy usable in muscular contraction?"

The theory of plant metabolism provides a striking example of an explanation of phenomena in terms of a substantial number of complex mechanisms. Calvin and Bassham, in their book on *The Photosynthesis of Carbon Compounds* [10], introduce a figure entitled "carbon reduction pathways in photosynthess" with the statement: "We believe the *principal* pathways for photosynthesis of simple organic compounds from CO_2 to be those shown in Figure 2." (Italics ours) The figure referred to represents more than forty distinct chemical reactions and a corresponding number of compounds. This diagram, of course, is far from representing the whole theory. Not only does it omit much of the detail, but it contains none of the quantitative considerations for predicting reaction rates, energy balances, and so on. The verbal description accompanying the figure, which also has little to say about the quantitative aspects, or the energetics, is over two pages in length—almost as long as our description of the chess-playing program. Here we have a clear-cut example of a theory of fundamental importance that has none of the parsimony we commonly associate with scientific theorizing.

The answer to the question of how photosynthesis proceeds is decidedly longwinded—as is the answer to the question of how chess players find mating combinations. We are often satisfied with such longwinded answers because we believe that the phenomena are intrinsically complex, and that no brief theory will explain them in detail. We must adjust our expectations about the character of information-processing theories of human thinking to a similar level. Such theories, to the extent that they account for the details of the phenomena, will be highly specific and highly complex. We might call then "garrulous theories" in contrast with our more common models of parsimonious theories.

Elementary Information Processes

We should like to carry the analogy with chemistry a step further. Part of our knowledge in chemistry—and a very important part for the experimental chemist—consists of vast catalogs of substances and reactions, not dissimilar in bulk to the compendium of information processes we are proposing. But, as we come to understand these substances and their reactions more fully, a second level of theory emerges that explains them (at least their general features) in a more parsimonious way. The substances, at this more basic level, become geometrical arrangements of particles from a small set of more fundamental sub-

stances—atoms and sub-molecules—held together by a variety of known forces whose effects can be estimated qualitatively and, in simple cases, quantitatively.

If we examine an information-processing theory like the checkmating program more closely, we find that it, too, is organized from a limited number of building blocks—a set of elementary information processes—and some composite processes that are compounded from the more elementary ones in a few characteristic ways. Let us try to describe these building blocks in general terms. First, we shall characterize the way in which symbols and structures of symbols are represented internally and held in memory. Then, we shall mention some of the principal elementary processes that alter these symbol structures [11].

Symbols, Lists, and Descriptions: The smallest units of manipulable information in memory are *symbol tokens* [12], or symbol occurrences. It is postulated that tokens can be compared, and that comparison determines that the tokens are occurrences of the same symbol (*symbol type*), or that they are different.

Symbol tokens are arranged in larger structures, called *lists.* A list is an ordered set, a sequence, of tokens. Hence, with every token on a list, except the last, there is associated a unique *next* token. Associated with the list as a whole is a symbol, its *name.* Thus, a list may be a sequence of symbols that are themselves names of lists—a list of lists. A familiar example of a list of symbols that all of us carry in memory is the alphabet. (Its name is "alphabet.") Another is the list of days of the week, in order—Monday is next to Sunday, and so on.

Associations also exist between symbol types. An association is a two-termed relation, involving three symbols, one of which names the relation, the other two its arguments. "The color of the apple is red" specifies an association between "apple" and "red" with the relation "color." A symbol's associations *describe* that symbol.

Some Elementary Processes: A symbol, a list, and an association are abstract objects. Their properties are defined by the elementary information processes that operate on them. One important class of such processes are the *discrimination* processes. The basic discrimination process, which compares symbols to determine whether or not they are identical, has already been mentioned. Pairs of compound structures—lists and sets of associations—are discriminated from each other by matching processes that apply the basic tests for symbol identity to symbols in corresponding positions in the two structures. For example, two chess positions can be discriminated by a matching process that compares the pieces standing on corresponding squares in the two positions. The outcome of the match might be a statement that "the two positions are identical except that the White King is on his Knight's square in the first but on his Rook's square in the second."

Other classes of elementary information processes are those capable of *creating or copying* symbols, lists, and associations. These processes are involved, for example, in fixating or memorizing symbolic materials presented to the sense organs—learning a tune. Somewhat similar information processes are capable of modifying existing symbolic structures by *inserting a symbol* into a list, by *changing a term of an association* (from "its color is red" to "its color is green"), or by *deleting a symbol* from a list.

Still another class of elementary information processes *finds* information that is in structures stored in memory. We can think of such a process, schematically, as follows: to answer the question, "What letter follows 'g' in the alphabet?," a process must find the list in memory named "alphabet." Then, another process must search down that list until (using the match for identity of symbols) it finds a "g." Finally, a third process must find the symbol *next* to "g" in the list. Similarly, to answer the question, "what color is the apple?," there must be a process capable of finding the second term of an association, given the first term and the name of the relation. Thus, there must be processes for finding named objects, for finding symbols on a list, for finding the next symbol on a list, and for finding the value of an attribute of an object.

This list of elementary information processes is modest, yet provides an adequate collection of building blocks to implement the chess-playing theory as well as the other information processing theories of thinking that have been constructed to date: including a general problem-solving theory, a theory of rote verbal learning, and several theories of concept formation and pattern recognition, among others [13].

Elementary Processes in the Chess Theory: A few examples will show how the mechanisms employed in the chess-playing theory can be realized by symbols, lists, associations, and elementary information processes. The player's representation of the chess board is assumed to be a collection of associations: with each square is associated the symbol representing the man on that square, and symbols representing the adjoining squares in the several directions. Moves are similarly represented as symbols with which are associated the names of the squares from which and to which the move was made, the name of the piece moved, the name of the piece captured, if any, and so on.

Similarly, the processes for manipulating these representations are compounded from the elementary processes already described. To make a move, for example, is to modify the internal representation of the board by deleting the association of the man to be moved with the square on which he previously stood, and creating the new association of that man with the square to which he moved; and, in case of a capture, by deleting also the association of the captured man with the square

on which he stood. Another example: testing whether the King is in check involves finding the square associated with the King, finding adjoining squares along ranks, files, and diagonals, and testing these squares for the presence of enemy men who are able to attack in the appropriate direction. (The latter is determined by associating with each man his *type*, and associating with each type of man the directions in which such men can legally be moved.)

We see that, although the chess-playing theory contains several thousand program instructions, these are comprised of only a small number of elementary processes (far fewer than the number of elements in the periodic table). The elementary processes combine in a few simple ways into compound processes and operate on structures (lists and descriptions) that are constructed, combinatorially, from a single kind of building block—the symbol. There are two levels of theory: an "atomic" level, common to all the information-processing theories, of symbols, lists, associations, and elementary processes, and a "macro-molecular" level, peculiar to each type of specialized human performance, of representations in the form of list structures and webs of associations, and of compound processes for manipulating these representations.

Processes in Serial Pattern Recognition: A second example of how programs compounded from the elementary processes explain behavior is provided by an information-processing theory of serial pattern recognition.

Consider a sequence like:

ABMCDMEFM——.

An experimental subject in the laboratory, asked to extrapolate the series will, after a little thought, continue:

GHM, etc.

To see how he achieves this result, we examine the original sequence. First, it makes use of letters of the Roman alphabet. We can assume that the subject holds this alphabet in memory stored as a list, so that the elementary list process for finding the NEXT item on a list can find B, given A, or find S, given R, and so on. Now we note that any letter in the sequence, after the first three, is related to previous letters by the relations NEXT and SAME. Specifically, if we organize the series into periods of three letters each:

ABM CDM EFM

we see that:

(1) The first letter in each period is NEXT in the alphabet to the second letter in the previous period.

(2) The second letter in each period is NEXT in the alphabet to the first letter in that period.

(3) The third letter in each period is the SAME as the corresponding letter in the previous period.

The relations of SAME and NEXT also suffice for a series like:

AAA CCC EEE . . .

or for a number series like:

1 7 2 8 3 9 4 0 . . .

In the last case, the "alphabet" to which the relation of NEXT is applied is the list of digits, 0 to 9, and NEXT is applied circularly—i.e., after 9 comes 0 and then 1 again.

Several closely related information-processing theories of human pattern recognition have been constructed using elementary processes for finding and generating the NEXT item in a list [14]. These theories have succeeded in explaining some of the main features of human behavior in a number of standard laboratory tasks, including so-called binary choice tasks, and series-completion and symbol-analogy tasks from intelligence tests.

The nature of the series-completion task has already been illustrated. In the binary choice experiment, the subject is confronted, one by one, with a sequence of tokens—each a "+" or "V," say. As each one is presented to him, he is asked what the next one will be. The actual sequence is, by construction, random. The evidence shows that, even when the subjects are told this, they rarely treat it as random. Instead, they behave as though they were trying to detect a serial pattern in the sequence and extrapolate it. They behave essentially like subjects faced by the series-completion task, and basically similar information-processing theories using the same elementary processes can explain both behaviors.

A Broader View of Thinking Processes

A closer look at the principal examples now extant of information-processing theories suggests that another level of theory is rapidly emerging, intermediate between the "atomic" level common to all the theories and the "macromolecular" level idiosyncratic to each. It is clear that there is no prospect of eliminating all idiosyncratic elements from the individual theories. A theory to explain chess-playing performances must postulate memory structures and processes that are completely irrelevant to proving theorems in geometry, and vice versa.

On the other hand, it is entirely possible that human performances in different task environments may call on common components at more aggregative levels than the elementary processes. This, in fact, appears to be the case. The first information-processing theory that isolated some of these common components was called the General Problem Solver [15].

Means-End Analysis: The General Problem Solver is a program organized to keep separate (1) problem-solving processes that, according to the theory, are possessed and used by most human beings of average intelligence when they are confronted with any relatively unfamiliar task environment, from (2) specific information about each particular task environment.

The core of the General Problem Solver is an organization of process for *means-end analysis.* The problem is defined by specifying a *given situation* (A), and a *desired situation* (B). A discrimination process incorporated in the system of means-end analysis compares A with B, and detects one or more *differences* (D) between them, if there are any. With each difference, there is associated in memory a set of *operators,* (O_D), or processes, that are possibly relevant to removing differences of that kind. The means-end analysis program proceeds to try to remove the difference by applying, in turn, the relevant operators.

Using a scheme of means-end analysis, a proof of a trigonometric identity like $\cos\theta \tan\theta = \sin\theta$ might proceed like this:

The right-hand side contains only the sine function, the left-hand side other trigonometric functions as well. The operator that replaces tan by sin/cos will eliminate one of these. Applying it we get $\cos\theta\,(\sin\theta/\cos\theta) = \sin\theta$. The left-hand side still contains an extraneous function, cosine. The algebraic cancellation operator, applied to the two cosines might remove this difference. We apply the operator, obtaining the identity $\sin\theta = \sin\theta$.

Planning Processes: Another class of general processes discovered in human problem-solving performances and incorporated in the General Problem Solver are *planning* processes. The essential idea in planning is that the representation of the problem situation is simplified by deleting some of the detail. A solution is now sought for the new, simplified, problem, and if one is found, it is used as a plan to guide the solution of the original problem, with the detail reinserted.

Consider a simple problem in logic. Given: (1) "A," (2) "*not A or B*," (3) "*if not C then not B*"; to prove "C." To plan the proof, note that the first premise contains A, the second A and B, the third, B and C, and the conclusion, C. The plan might be to obtain B by combining A with (AB), then to obtain C by combining B with (BC). The plan will in fact work, but requires (2) to be transformed into "A implies B" and (3) into "B implies C," which transformations follow from the definitions of "or" and "if . . . then."

Problem-Solving Organization: The processes for attempting subgoals in the problem-solving theories and the exploration processes in the chess-playing theory must be guided and controlled by executive processes that determine what goal will be attempted next. Common principles for the organization of the executive processes have begun

to appear in several of the theories. The general idea has already been outlined above for the chess-playing program. In this program the executive routine cycles between an exploration (*search*) phase and an evaluation (*scan*) phase. During the exploration phase, the available problem-solving processes are used to investigate subgoals. The information obtained through this investigation is stored in such a way as to be accessible to the executive. During the evaluation phase, the executive uses this information to determine which of the existing subgoals is the most promising and should be explored next. An executive program organized in this way may be called a search-scan scheme, for it searches an expanding tree of possibilities, which provides a common pool of information for scanning by its evaluative processes [16].

The effectiveness of a problem-solving program appears to depend rather sensitively on the alternation of the search and scan phases. If search takes place in long sequences, interrupted only infrequently to scan for possible alternative directions of exploration, the problem solver suffers from stereotypy. Having initiated search in one direction, it tends to persist in that direction as long as the subroutines conducting the search determine, locally, that the possibilities for exploration have not been exhausted. These determinations are made in a very decentralized way, and without benefit of the more global information that has been generated.

On the other hand, if search is too frequently interrupted to consider alternative goals to the one being pursued currently, the exploration takes on an uncoordinated appearance, wandering indecisively among a wide range of possibilities. In both theorem-proving and chess-playing programs, extremes of decentralized and centralized control of search have shown themselves ineffective in comparison with a balanced search-scan organization.

Discrimination Trees: Common organizational principles are also emerging for the rote memory processes involved in almost all human performance. As a person tries to prove a theorem, say, certain expressions that he encounters along the way gradually become familiar to him and, his ability to discriminate among them gradually improves. An information-processing theory (EPAM) was constructed several years ago to account for this and similar human behavior in verbal learning experiments (e.g., learning nonsense syllables by the serial anticipation or paired associate methods) [17]. This theory is able to explain, for instance, how familiarity and similarity of materials affect rates of learning. The essential processes in EPAM include: (1) processes for discriminating among compound objects by sorting them in a "discrimination tree," (2) familiarization processes for associating pairs or short sequences of objects.

Discrimination processes operate by applying sequences of tests to

the stimulus objects, and sorting them on the basis of the test results—a sort of "twenty questions" procedure. The result of discrimination is to find a memory location where information is stored about objects that are similar to the one sorted. *Familiarization processes* create new compound objects out of previously familiar elements. Thus, during the last decade, the letter sequence "IPL" has become a familiar word (to computer programers!) meaning "information processing language." The individual letters have been *associated* in this word. Similarly, the English alphabet, used by the serial pattern-recognizing processes, is a familiar object compounded from the letters arranged in a particular sequence. All sorts of additional information can be associated with an object, once familiarized. (For example, the fact that IPL's organize symbols in lists can be associated with "IPL".)

Because discrimination trees play a central role in EPAM, the program may also be viewed as a theory of pattern detection, and EPAM-like trees have been incorporated in certain information-processing theories of concept formation. It also now seems likely that the discrimination tree is an essential element in problem-solving theories like GPS, playing an important role in the gradual modification of the subject's behavior as he familiarizes himself with the problem material.

Conclusion

Our survey shows that within the past decade a considerable range of human behaviors has been explained successfully by information-processing theories. We now know, for example, some of the central processes that are employed in solving problems, in detecting and extrapolating patterns, and in memorizing verbal materials.

Information-processing theories explain behavior at various levels of detail. In the theories now extant, at least three levels can be distinguished. At the most aggregative level are theories of complex behavior in specific problem domains: proving theorems in logic or geometry, discovering checkmating combinations in chess. These theories tend to contain very extensive assumptions about the knowledge and skills possessed by the human beings who perform these activities, and about the way in which this knowledge and these skills are organized and represented internally. Hence, each of these theories incorporates a rather extensive set of assumptions, and predicts behavior only in a narrow domain.

At a second level, similar or identical information-processing mechanisms are common to many of the aggregative theories. Means-end analysis, planning, the search-scan scheme, and discrimination trees are general-purpose organizations for processing that are usable over a wide range of tasks. As the nature of these mechanisms becomes better un-

derstood, they, in turn, begin to serve as basic building blocks for the aggregative theories, allowing the latter to be stated in more parsimonious form, and exhibiting the large fraction of machinery that is common to all, rather than idiosyncratic to individual tasks.

At the lowest, "atomic," level, all the information-processing theories postulate only a small set of basic forms of symbolic representation and a small number of elementary information processes. The construction and successful testing of large-scale programs that simulate complex human behaviors provide evidence that a small set of elements, similar to those now postulated in information-processing languages, is sufficient for the construction of a theory of human thinking.

Although none of the advances that have been described constitute explanations of human thought at the still more microscopic, physiological level, they open opportunities for new research strategies in physiological psychology. As the information-processing theories become more powerful and better validated, they disclose to the physiological psychologist the fundamental mechanisms and processes that he needs to explain. He need no longer face the task of building the whole long bridge from microscopic neurological and molecular structures to gross human behavior, but can instead concentrate on bridging the much shorter gap from physiology to elementary information processes.

The work of Lettvin, Maturana, McCulloch, and Pitts on information processing in the frog's eye[18], and the work of Hubel and Wiesel on processing of visual information by the cat [19] already provide some hints of the form this bridging operation may take.

APPENDIX: COMPUTER PROGRAMS AS THEORIES

Since the use of computer programs as formal theories, in the manner described in this paper, is still somewhat novel, this appendix sketches briefly the relation between this formalism and the formalisms that have been used more commonly in the physical sciences.

In the physical sciences, theories about dynamical systems usually take the form of systems of differential equations. This is the form of classical Newtonian mechanics, of Maxwell's electromagnetic theory, and of many other theories of central importance. In the classical dynamics of mass points, for example, it is assumed that the initial positions and velocities of a set of bodies (mass points) are given, and that the forces acting on the bodies are known, instantaneous functions of the positions, say, of the bodies. Then, by Newton's Second Law, the acceleration (second derivative of position) of each body is proportional to the resultant force acting on it. The paths of the bodies over time are calculated by integrating twice the differential equations that express the Second Law.

More generally, a system of differential or difference equations is a set of conditional laws that determines the state of the system "a moment later" as a function of its state at a given time. Repeated application of the laws, equivalent to integrating the equations, then determines the path of the system over time.

A computer program is also literally a system of difference equations—albeit of a rather unorthodox kind. For it determines the behavior of the computer in the next instruction cycle as a function of the current contents of its memory. Executing the program is formally equivalent to integrating (numerically) the difference equations for a specified initial state of the computer. Thus, information-processing theories, expressed as programs in computer languages, are not merely analogous to more familiar kinds of dynamical theories; formally, they are of an equivalent type.

Very simple systems of differential and integral equations can sometimes be integrated formally, so that general properties can be inferred about the paths of the systems they describe, independent of particular initial and boundary conditions. There are no known methods for integrating formally systems of difference equations like those discussed in this paper. Hence, the principal means for making predictions about such systems is to simulate their behavior for particular initial and boundary conditions. This is the method of investigation that we have relied on here.

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BEHAVIOR OF AIRBORNE FISSION PRODUCTS

By LUTHER B. LOCKHART, JR.

In a report published in this journal several years ago [1], the authors speculated on the behavior of long-lived radioactive fission products in the atmosphere during an extended period free of nuclear testing. Several postulations were made regarding the degree of pollution of the lower atmosphere to be expected at various times during the moratorium, dependent on the rate of interhemispheric mixing and of stratospheric deposition. Though some consideration was given to the concept of a seasonal variation in the rate of subsidence of radioactivity from the stratosphere, the magnitude of this factor was not really appreciated at that time.

The present report is intended to give an accounting of some experimental results actually obtained from atmospheric measurements made during the period 1958–1962 using the techniques described earlier. The data have been obtained from the 80th Meridian Air Sampling Program operated by the U.S. Naval Research Laboratory with the cooperation of a number of agencies and organizations in North and South America. The work has been financed, in part, by the Division of Biology and Medicine of the Atomic Energy Commission.

Air filters were exposed at each site by the cooperating group, returned to the U.S. Naval Research Laboratory by air and assayed for gross β-activity two weeks following the end of the collection period. Radiochemical analyses for a number of isotopes, including Sr^{90}, were later run.

The radioactivity burden of the troposphere as a function of time has been estimated from the average air concentrations of activity found at ground level at the various sites by assuming a uniform volume concentration up to the height of the tropopause and weighting by the areas associated with the various latitude bands. Seasonal variations in the average location of the tropopause have also been taken into account. There are problems, certainly, associated with the above assumptions and with the use of a single meridian as representative of global behavior. The general picture of the behavior of radioactive bomb debris in the atmosphere should be reasonably correct, however.

The monthly averages of the burdens of gross β-activity associated with particulate radioactive debris from bomb tests are presented in Figure 1 for the period 1958–1962. This period includes the nearly three-year moratorium on nuclear testing by the United States, the United Kingdom, and the Soviet Union sandwiched between two active periods

of testing of large yield devices. Only the elapsed time between the major test series is of value in the estimation of the stratospheric depletion rate; the periods of intensive testing, however, with the quite different tropospheric loadings in the two hemispheres, did demonstrate that the equatorial region effectively hindered the interhemispheric mixing of particulate radioactivity in the lower atmosphere.

The tropospheric components of radioactivity produced in the various bomb tests were evidenced by immediate increases in the tropospheric burdens of gross β-activity, as, for example, that observed following the first French test in the Sahara on February 13, 1960. In this case, cross-over into the Southern Hemisphere as far south as Antofagasta, Chile

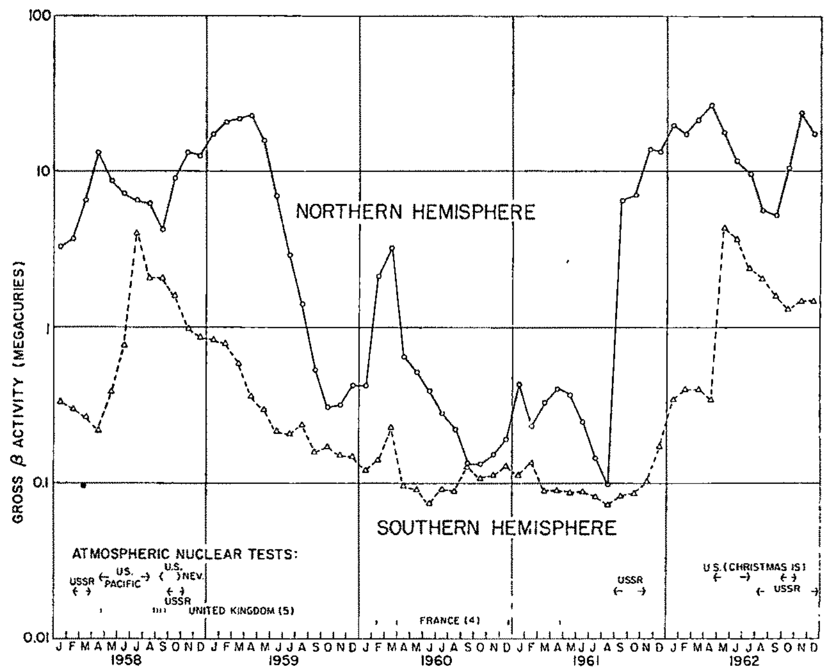

FIG. 1. Tropospheric Burdens of Gross Fission Product β-Activity, 1958–1962.

(23° 37′S) was documented by radiochemical analyses. Detailed interpretation of the air concentrations and burdens of gross β-activity is not warranted because of the uncertain factor introduced by radioactive decay of the fission products. This decay rate is a function of the age of the debris; in mixtures consisting of debris from a multiplicity of sources, it is difficult to make reasonable assignments of ages to the various components.

Sr^{90} on the other hand, because of its long half-life (27.7 years),

serves as a conservative tracer for the movement and disposition of bomb debris in the atmosphere. Thus, the burdens of Sr^{90} in the troposphere can, in the absence of fresh nuclear debris there, be directly related to the balance between subsidence from the stratosphere and deposition on the earth's surface. Since fresh fission product conglomerates are always extremely poor in Sr^{90} relative to the total β-activity and since essentially complete washout or deposition of the tropospheric debris occurs within a matter of months, the small French tests during the moratorium had a negligible effect on the over-all picture.

The Sr^{90} results plotted in Figure 2 show the strong influence of

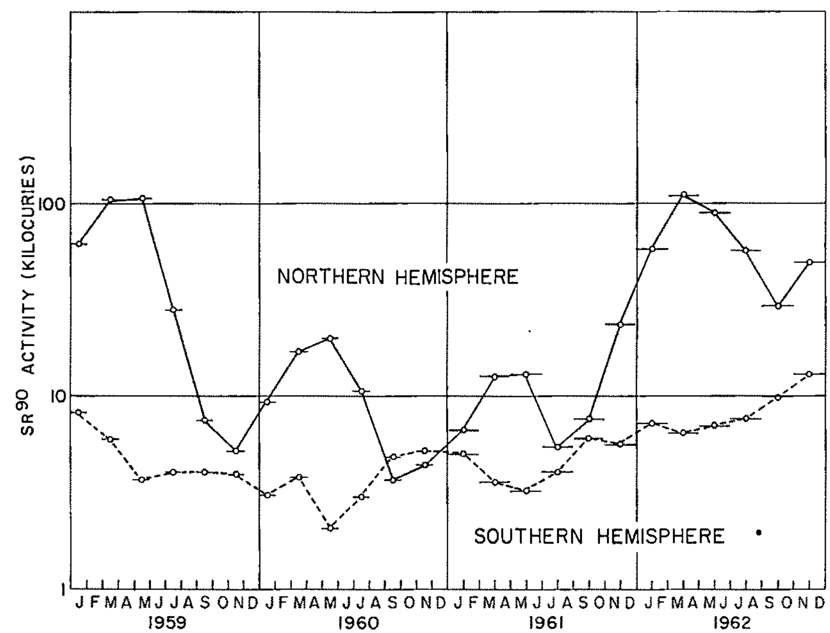

FIG. 2. Burdens of Sr^{90} in the Troposphere, 1959–1962.

seasonal factors on radioactive fallout. The peaks are directly related to the greatly increased rate of downward mixing during the winter-spring season of stratospheric air rich in the long-lived fission products. Seasonal changes in rainfall rates, particularly in the tropical regions, influence the Sr^{90} burdens only to a minor extent. Measurements of the accumulation of Sr^{90} on the earth's surface, made by other investigators, likewise exhibit seasonal increases that are in phase with the maxima shown here.

In the Northern Hemisphere, the large quantities of Sr^{90} found in the air at ground level in early 1959 resulted principally from tests held in the Soviet Union during the fall of 1958, with some contribution from the U.S. Pacific tests of 1958 and from earlier test series. That found during 1960 and 1961 was a mixture from many tests with no large proportion

assignable to any particular test series. The fast rate of decrease between the maxima of 1959 and of 1960 is thus due primarily to the short residence time of debris injected into the polar region, and suggests an average residence time there of less than six months. A similar consideration of the 1960–1961 activity burdens leads to an estimated average lifetime of nearly four years for the remaining stratospheric activity, primarily that resulting from injections in the tropical regions. This latter figure appears to be too high, though, for several reasons.

Measurements of the W^{185} produced as a tracer in a number of the U.S. HARDTACK tests in the Pacific (April–August 1958) indicated a reduction of Sr^{90} in the ground-level air from this source by about 75 per cent between 1959 and 1960, corresponding to an average residence time of less than a year for these tropically injected debris. Measurements by other investigators of another tracer, Rh^{102}, produced in the TEAK and ORANGE high altitude tests fired by the U.S. in August 1958, have indicated the increasing influx during 1961 of debris from this source. This new component thus caused only an apparent increase in the overall residence time. It is evident, therefore, that both the altitude and the latitude of the injection of bomb debris are important in determining the residence time, and that the idea of a general stratospheric residence time has little meaning.

In the Southern Hemisphere, for some reason as yet not understood, the seasonal variation is not well developed. Some moderation of the amplitude of the cycle could result from the equatorial crossover of debris from the Northern Hemisphere during its spring maximum, which would tend to cancel out the Southern Hemisphere fall minimum. Measurements made during 1962, when a large imbalance between hemispheres existed, would tend to discredit the significance of this mechanism. A more logical explanation would be the lack of any quantity of debris in the antarctic stratosphere in a position to undergo strong seasonal variations in downward mixing, since no significant stratospheric injections have been made south of the Christmas Island area (2°N). This unsymmetrical distribution could also account for the lack of a well-developed spring peak at sites in southern Chile, where the damping effects of equatorial crossover of tropospheric debris should be minimal. Furthermore, comparisons of radioactivity levels at comparable latitudes north and south of the Equator show a decided shortage in the higher latitudes of the Southern Hemisphere.

One other speculation discussed in the previous paper was the possible estimation of the rate of transequatorial movement of radioactivity within the stratosphere. Since our simplified picture of the atmosphere has already been demolished, perhaps the best we can do here is to revise our speculation. There is a real and progressive increase in the Sr^{90} burden of the ground-level air in the Southern Hemisphere that is sug-

gestive of southward mixing of material from the Northern Hemisphere stratospheric source, at a rate that exceeded the rate of depletion of the Southern Hemisphere source; moreover, the hemispheric burdens were seemingly approaching equivalent but out-of-phase values prior to resumption of the large scale testing in September 1961. Questions already raised regarding the importance of the position and altitude of the stratospheric sources prevent any very definite conclusions to be drawn from such ground-level measurements regarding the behavior of the stratospheric debris. Such programs as the Atomic Energy Commission, the Weather Bureau, and the Defense Atomic Support Agency have sponsored on the collection of stratospheric radioactivity through balloon-borne and aircraft samplers or on the *in situ* measurement of stratospheric radioactivity by balloon-borne γ-counters, have shown that equatorial crossover does occur and that, in general, gross movement in the stratosphere in a north-south direction appears to be primarily by a diffusion mechanism rather than by an organized meridional circulation.

It is unfortunate that the extensive nuclear test programs carried out in 1961–1962 prevented a continuation of this study. Verification of the present conclusions by observations during the coming years free of large-scale atmospheric testing is certainly indicated, because of the perverse and often inconsistent nature of atmospheric processes.

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SALT TRANSPORT ACROSS CELL MEMBRANES*

By STERLING B. HENDRICKS

HETEROGENEITY is universal among organisms. The cell has an integrity of its parts and retains even small molecules while interchanging others between the parts and the surroundings. The parts, and the cells themselves, are set off by walls or thin membranes having an inside, an outside, and some manner of permeation. I am concerned with the permeation, or membrane transport, for salts.

Many cells select between ions as closely similar as Na^+ and K^+, or even K^+ and Rb^+. They can establish salt concentrations in excess of the ambient solutions; for instance, in the gastric secretion in the stomach mucosa and in cells of fresh-water algae. The process of ionic exchange plays a part in the action of biological transducers, like muscles and nerves, where one type of action is transformed to another; chemical to mechanical or chemical to electrical in these instances.

I work with the dependence of plant growth on soils, features of which are the transport of water and accumulation of salts by the root from the dilute solutions in the soil. Accumulation by the root is a determinative factor in the fertility of soils, the use of fertilizers and lime, and the effects of excess salts in saline waters on plant growth. A comparative approach has been followed in the study of these phenomena. It leads to two questions: Does salt absorption by roots in soil resemble transport through the skin of a frog in a pond? What are the similitudes in all salt transport?

An approach to the second question, which includes the first one as a special case, is the purpose of this article. But I hesitate, knowing that salt transport in plants is technically involved, as also are the methods of physiology. Someone familiar through long experience with muscle fibers or red blood cells might fall into simple traps about plants. The hazards in the reverse direction are no less real, nor is the necessity of dealing with both physical and chemical phenomena of transport. Nevertheless, the effort is worth the candle.

Properties of Water and of Uncharged Holes

A property of all membranes across which salt transfer can occur is permeability to water. This is a first and simplest indication that the extensive material of the membranes is either pervious or broken at intervals by channels or holes containing water. The passage of water is

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greatly restricted, which implies that channels if present are a minor part of the entire surface and have a resistance to flow. This implies cross sections of the holes approaching atomic dimensions, imparting sieve-like properties to the living membranes. The membranes can be used as osmometers allowing waterflow while preventing transport of solutes. In fact, the membrane used by Abbé Nollét in 1748 in the first

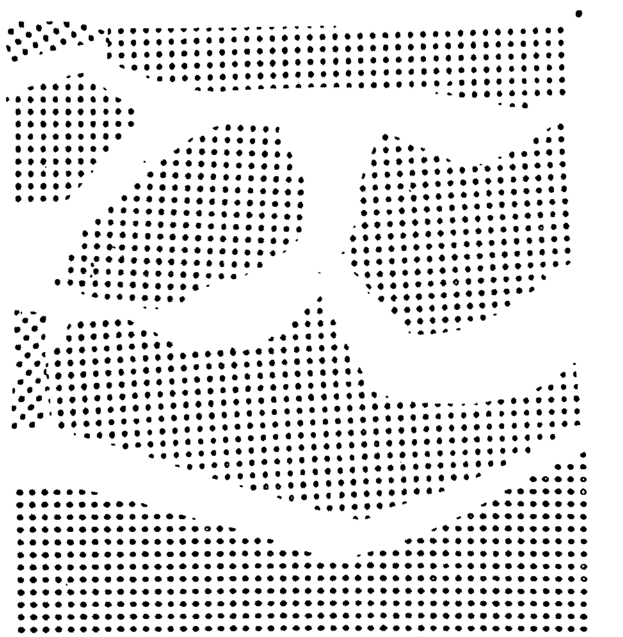

FIG. 1. Schematic pattern of holes in an artficial membrane.

experiments on osmosis was an animal bladder separating a solution of alcohol in water from water, with resultant slow dilution of the solution. The holes in the bladder must have cross sections of the order of the dimensions of ethanol. Properties of holes are the starting point of a comparative approach to salt transport without commitment about a possible transmitting extensive phase.

Three properties of holes are cross section, length, and tortuosity. These properties are well displayed by artificial membranes made from collodion. The solutions of cellulose nitrate in a volatile solvent are spread in thin layers and allowed to evaporate. A brush pile-like array of fibrils remains with holes of various sizes (Fig. 1). Permeabilities of these collodion membranes for some nonelectrolytes in water solution were measured after both drying in air and swelling in alcohol, with the following results (Collander, 1926):

Substance	Molecular refractivity for 5778 Å as a measure of molecular size	Relative permeability dried membrane	swollen membrane
Ammonia	5.8	25.0	100
Formic acid	8.6	7.3	86
Ethanol	12.8	1.6	
Lactic acid	19.2	0.2	30
Sucrose	70.3	0.0	1.6

The permeability depends upon the molecular size, or conversely upon the dimensions of the holes and the distribution of their cross sections. The minimum cross sections of excluded sucrose molecules are about 8 × 10 Å, and ammonia, which is readily admitted, is about 3 Å in diameter. The currently developed dextrans used in protein and other molecular separations, which are cross linked chains of glucose groups in a space array, available under the tradename Sephadex, also illustrate the sieving properties of holes.

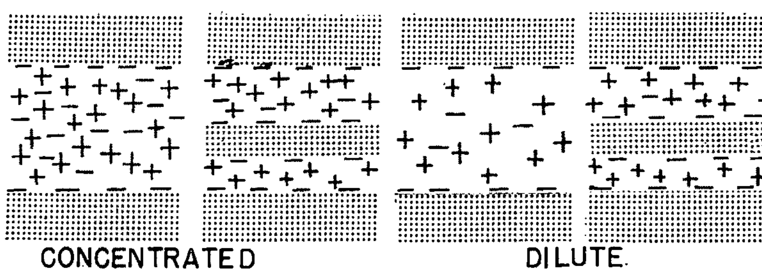

FIG. 2. Possible pattern for distribution of ions at two concentrations in membrane holes with charged walls (after Sollner, 1958).

Properties of holes include the properties of their walls. Collodion walls can be charged by incorporation of polymeric ions or by introduction of carboxylic groups through oxidation with nitric acid. Other polymers containing cationic and anionic groups are important as membranes and porous solids in bulk. The ionizable groups in the walls of the holes have specific properties toward salt solutions. The holes in natural membranes appear to be similar in type, with both cations and anions in the walls.

The basic principles of charged holes have been closely studied. I paraphrase the treatment of Dr. Karl Sollner (Sollner, 1958), as well as draw upon my own knowledge of ionic exchange. Ions in walls must have ions of matching charges in the holes leading to the distribution of the types shown in Figure 2. The number of positive and negative charges, including the ones in the walls, must be equal. Ions similar in charge to the ones in the walls are repelled and, if the cross sections of the

holes are sufficiently small, they are virtually excluded. In this circumstance, the number of charges in the holes, and counter ions, are equal to the charge in the wall or the number of ions are equal if they both are singly charged. If transport from one side to the other takes place, only the counter ions can move and the membrane will have selective permeability for them; thus K^+ might be transported and Cl^- excluded. The distance between the ions in the larger holes depends on the concentration of the solution, Figure 2, decreasing as the concentration increases.

Properties of Charged Holes

Electrical phenomena are associated with membranes. If salt solutions of different concentrations are on the two sides of the membrane, they will be in contact in the holes and a potential will be established in the contact region because of differences in mobilities of the migrating ions. This potential, E, is

$$E = \frac{\tau_+ - \tau_-}{\tau_+ + \tau_-} \frac{RT}{F} \ln \frac{a_1^{\pm}}{a_2^{\pm}}$$

in which τ's are transference numbers in the membrane and a's are salt activities in the two solutions. If the holes are small enough to allow only the oppositely charged counter ions to pass, then either τ_+ or τ_- is zero and $\tau_+ - \tau_-/\tau_+ + \tau_-$ is $+1$ or -1. In this extreme E becomes

$$E = \frac{RT}{F} \ln \frac{a_1^+}{a_2^+} \text{ or } \frac{-RT}{F} \ln \frac{a_1^-}{a_2^-}$$

which is an equation derived by Nernst, familiar as the equation for measuring H^+ activity when an electrode reversible to H^+ is used.

Measurement at 25.00° on collodion membranes (Sollner, 1958) containing sulfonated polystyrene in the acid form illustrates these membrane properties:

Cation exchange capacity, meq/cm²	*Water content, %*	*Avg resist in 0.1 M KCl, Ω/cm²*	*Avg potential 0.4/0.2 M KCl, mv*	*Avg rate of osmotic water movement with 0.2 M sucrose mm³/h/100 cm²*
0.32	5.6	2030	15.45	8.4
0.50	5.7	435	15.48	13.7
0.69	7.0	54.5	15.32	57
1.14	10.3	26.3	15.09	190
3.20	26.8	18.5	13.9	2000

The theoretical potential calculated from the Nernst equation for 0.4 M KCl on one side of the membranes and 0.2 M on the other, as used under the measuring conditions, is 15.95 millivolts. The theoretical value

is approached if the holes are restricted in cross section as shown by the lower water content, exchange capacity, and rate of osmotic water movement, and the higher resistance per unit area. The potential decreases, as shown in the last row, when the holes are quite large, which is reflected in rapid osmotic water movement. The potential across a given membrane in the preceding series depends on the concentrations of the solutions and decreases when these are sufficiently great, for the reasons illustrated in Figure 2.

The particular membranes permit transport of cations, K^+, but not anions, Cl^-, and thus have selective permeability. But they do not differentiate particularly between K^+ and Na^+. Such differentiation involves another principle determining the properties of charged holes. It has to do with differences between ions of the same charge, Na^+, K^+, or Cl^-, Br^-, or ions of like charge, K^+, Ca^{++}, or Cl^-, SO_3^{--}. A counter ion migrating through a hole in a membrane of selective permeability has to pass along from one ion in the wall to another. There is a force, approximately the electrostatic attraction, between each of the ion pairs and this must, in part, be overcome for transport to take place. Looked at in another way, there is a potential barrier between each wall site for a migrating ion. The transport will be limited both by the specificity with which a counter ion is held by each wall charge and by the height of the potential barrier. These can be controlled in artificial membranes.

Various kinds of anionic or cationic groups can be incorporated into the artificial membrane and appear on the walls of the hole. These groups, in general, will have different interactions with counter ions. More of one than another of two competing ions will be present as counter ions in contact with a solution having equal activities of the two ions. The differences, in general, are not great and the favorable factor at one ionic position might be only 1.414 fold, as an example. If the holes are sufficiently restricted in cross section, however, one ion can only exchange with another down the length of the hole. Under this condition the favorable factor is enhanced by $(1.414)^n$ where n is the number of exchanges. If n is 20, which is not unreasonable, the favorable factor is $(1.414)^{20}$ or 1024. This enhancement can only be attained when transport is occurring—that is, dynamically.

If two salts, A^+C^- and B^+C^-, are separated by a membrane permeable to A^+ and B^+ but impermeable to C^-, thus

A^+C^-	Cation permeable selective membrane	B^+C^-

a potential will be established given by

$$E = \frac{RT}{F} \ln \frac{\tau_{A^+}}{\tau_{B^+}} \frac{a_{A^+}}{a_{B^+}}$$

In this equation, τ's are transference numbers in the membrane and a's

are activities (Michaelis, 1929). A similar equation with a negative sign holds for anions across an anion-selective membrane. Protoamine collodion membranes, which are anion-selective, give values of $E = -46.2$ mv for 0.025 M NaCl/0.025 M Na acetate, corresponding to $\tau_{Cl^-}/\tau_{acetate^-} = 0.165$ (Sollner, 1958). Membranes, or porous solids of this general type, are coming into wide use for selective separation of salts. Their properties are strikingly similar to those of natural membranes.

Osmosis and the Donnan equilibrium (Donnan, 1911) are other phenomena associated with membranes across which water can diffuse but transport of solutes is hindered. If there is a difference of numbers of molecules on two sides of a membrane permeable to water but impermeable to solute, the water will move toward equilibrium or, conversely, a pressure will exist at any moment on the side with the greatest amount of solute relative to that with the least. If the containing volumes are free to change as water is transported, swelling or shrinking will take place and the result is breakage, or plasmolysis, in the limit for natural membranes. Animal cells, in general, approach isotonicity with respect to the ambient solution. If they cannot adjust solute content as quickly as water content, they break upon dilution of their ambient media. Plant cells, animal epithelia, and most free-living unicellular forms have strong walls that resist outward pressures and thus do not change volume appreciably even when these pressures are large.

Donnan equilibria arise if both small diffusible and large nondiffusible ions are present. The counter ions are restricted to the neighborhood of the nondiffusible ions and the remainder of the salt diffuses to equilibrium. A positive osmotic pressure is produced on the side of the greatest nondiffusible ion concentration, which must be under hydrostatic pressure with a difference in potential having the same sign as the nondiffusible ion. The distribution is

$$2c_i^{\pm} = \sqrt{n_i^2 + 4c_0^2} \pm n_i$$

If the external salt concentration, c_0, is low compared with that of the nondiffusible ion, n_1, the inside ion concentration c_0 approaches zero. When c_0 is comparatively large, c_i can be an appreciable fraction of n_0 which it approaches as a limit for n equal to zero.

An Aspect of Irreversible Thermodynamics

The general equations for transport and equilibration of salts across membranes with holes can be formulated by principles of irreversible thermodynamics. The irreversibility arises from entropy increases or free-energy changes taking place in the membrane in a nonreversible way. The total volume flow through the membrane, J_v, and solute flow, J_s, are

$$J_v = L_p \cdot \Delta p + L_{pD} \cdot \Delta \pi$$
$$J_s = L_{pD} \cdot \Delta p + L_D \cdot \Delta \pi$$

where Δp and $\Delta \pi$ are the pressure and osmotic pressure differences and L_p and L_D are the mechanical coefficient of filtration and a diffusion coefficient, respectively. The cross coefficient L_{pD} is the volume flow due to unit osmotic pressure when $\Delta p = 0$ and the solute flow when $\Delta \pi = 0$. In coarse membranes, neither osmotic flow nor filtration takes place, so $L_{pD} = 0$. If the membrane is an ideal semipermeable one, $L_{pD} = -L_p = -L_D$. The ratio L_{pD} to L_p has been called the reflection coefficient, σ

$$\sigma = \frac{-L_{pD}}{L_p}$$

where σ is between 0 and 1. The coefficients L can best be interpreted in terms of frictional forces and, when this is done, the reflection coefficient σ can be expressed as

$$\sigma = 1 - \frac{\omega \bar{v}_s}{L_p} - \frac{D}{D_0}$$

with

$$\omega = \frac{D\ \varphi}{RT\Delta x}$$

where φ is the fractional area of the holes, Δx the thickness of the membrane, $\bar{v}_s$ is the partial molal volume of the solute, L_p is the volume flow when concentrations are equal on the two sides, and D and D^0 are diffusion coefficients for the solute in the hole and the free solution, respectively (Katchalsky, 1961). With charged holes the equation for σ becomes

$$\sigma = 1 - \frac{\omega\ \bar{v}_s}{L_p} - \frac{\omega \Delta x}{\varphi} \cdot f_{sw}$$

where f_{sw} is a frictional coefficient given by $F_{sw} = -f_{sw}\ (v_s - \bar{v}_w)$, F_{sw} being the frictional force between a mole of solute and the surrounding water.

The value of σ has been measured for two unicellular algae (Dainty, 1961) and for erythrocytes (Solomon, 1961). In the algae, σ has values near 0.3 for passage of methanol, ethanol, and isopropanol, indicating that the last term in the equation is an appreciable fraction of 1.0, because of f_{sw}, arising from interaction of water and the alcohol in the holes, the existence of which in the membrane is supported. The value of σ obtained with erythrocytes was reduced to an equivalent pore radius and found to be 4.2 Å, which is about the radius of an hydrated Na^+ ion. A larger value, 5.6 Å, was obtained with *Necterus* kidney slices where the pores were predominantly those of proximal tubule cells (Whittembury, *et al.*, 1960).

Natural Membranes

Natural membranes display all the properties of artificial ones. In general, however, they are much thinner, being supported by the turgor

pressure of enclosed liquids. Their continuous phases are more permeable to nonelectrolytes and might be less impervious to ionic compounds. The continuous phases of natural membranes contain proteins, nucleic acids, polysaccharides, and lipids as predominant components. The first three, as polymers of high molecular weight, form a thin framework of considerable rigidity. The proteins, which are mostly insoluble, are in part lipoproteins and are conjugated to some extent with carbohydrates. They have a number of enzymatic activities, the ones to which most attention has been devoted being phosphotases, including ones catalyzing hydrolysis of adenosine triphosphate, and dehydrogenases such as triose phosphate dehydrogenase. The membranes of mitochondria are of particular interest as carriers of the electron transport system with which oxidative phosphorylation is coupled.

The lipids form about 10 to 25 per cent of the membrane constitutents and impart the essential lack of permeability to ionic substances. They are chiefly glycerol esters containing two fatty acid residues and one phosphate group which is also esterfied with the bases choline or ethanolamine (Frazer, 1963). These phospholipids account for more than 75 per cent of the total lipid, other components of which are various sterols. The predominant esterifying fatty acids contain 14 to 18 carbon atoms and more than 50 per cent are unsaturated. It is pertinent to note that the resistance to evaporation of water caused by a surface monolayer of fatty acid is greatly reduced by small amounts of unsaturated fatty acids which produce flaws in an otherwise regular array (La Mer, 1962). These unsaturated compounds also greatly enhance water transport across root cell membranes (Kuiper, 1964). The lipids of stroma of human erythocytes (Prankerd, 1961) and mitochondrial walls (Green and Fleischer, 1963) illustrate detailed studies.

Electron micrographs (Fig. 3) show stained membranes as thin films that almost always appear to be double layers (Harris, 1960). Many pictures have been drawn on a molecular scale of the protein and lipid distribution in these layers. The proteins are the more hydrophilic of the two because of their polar and ionic groups. For this reason they are often considered to be on the outside of both layers (Green and Fleischer, 1963). The non-polar side chains of the protein, the hydrocarbon parts of the constituent amino acids, are thought to face inward and to associate with the long chain fatty acid parts of the lipids. A diversity of pattern is conceived for the arrangement of the lipid but a general feature is a smetic or two-dimensionally ordered array (Danielli and Davson, 1935) leading to an observable birefrigence. Some of the ionic groups must be buried in the water-repellant part of the film. A double membrane of this type is sketched on a molecular scale in Figure 4.

Double membranes form in lipid water systems and are seen on a microscopic scale in ternary systems consisting of soap-water-amphiphile, the

last being a substance such as a sterol with a polar group and a considerable hydrocarbon part (Lawrence, 1958). A manner in which holes might form in a protein lipid membrane is shown in Figure 4 (Wolman and Wiener, 1963). The sodium salt of the lipid form from myelin of a nerve sheath forms a hydrophilic exterior, with the formation

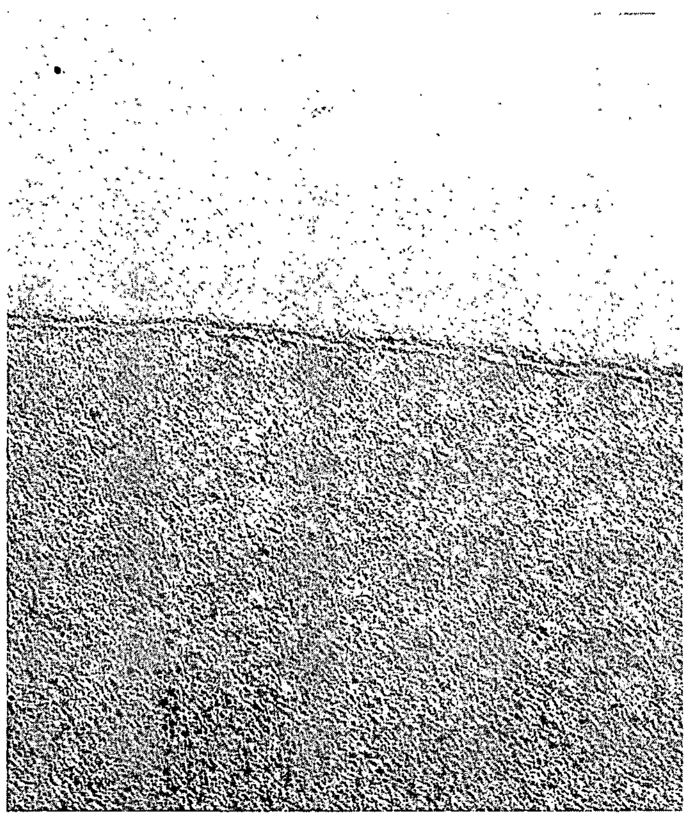

FIG. 3. An electron micrograph of a segment of a human red blood cell membrane fixed with $KMnO_4$ and sectioned transversely (350,000×) (courtesy J. D. Robertson).

of resultant water channels through the membrane. Calcium salts of the membrane components, on the other hand, maintain lipid soluble properties and thus tend to preserve the double film (Clowes, 1916). In any case, transport across living membranes is markedly changed by Mg^{++} and Ca^{++} as contrasted with Na^{+} and K^{+}. The property of modification is an important distinction between the natural and artificial membranes.

The lipid-protein continuous phase is permeable to some nonpolar substances somewhat soluble in both water and lipid. Among these are alcohols and other oxygen-containing nonionic compounds. Organic car-

boxylic acids such as acetic, succinic, and malonic are representative. These are taken up by plant cells under acid conditions where the undissociated acid is predominant. Usually, when they are to serve as substrates or inhibitors of cell metabolism they must be supplied in acid solution, that is, unionized, to enter the cells.

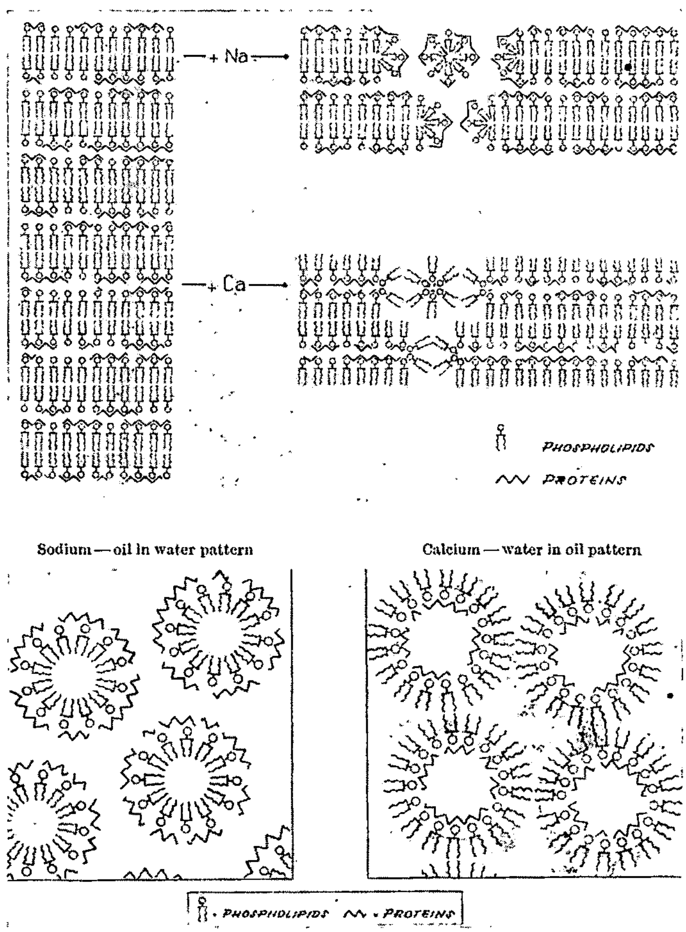

FIG. 4. Possible effects of Na^+ and Ca^{++} on lipid-protein membranes (after Wolman and Wiener, 1963, courtesy M. Wolman and *Nature*, London).

Reactions can take place in both natural and artificial membranes. While they are restricted to the holes in the latter, both the ions of the hole walls and components or enzymes in the extensive lipid-protein phase can be involved in the natural membranes. Salt accumulation against an electrochemical potential difference must use the free-energy change in these reactions to accomplish the increased electrochemical potential of

the salt. Ionic selectivity is also a dynamic result, depending in artificial membranes upon prior exchange with ions other than the one in question and in natural membranes on acid-producing reactions. The selectivity alone, however, cannot lead to accumulation against the electrochemical potential. The types of reactions and concepts about their manners of coupling will be discussed under the several biological examples of salt transport.

A general technique has greatly facilitated measurement of salt transport. This is radioactive tracing which was first described in 1923 by Hevesy (Hevesy, 1923) in an article entitled "The absorption and translocation of lead by plants: A contribution to the application of the method of radioactive indicators in the investigation of changes of substances in plants." Hevesy used thorium B, a natural isotope of lead (Pb^{212}) with a half-life of 10.6 hours, to follow the absorption and distribution of lead in bean plants growing in solutions of various concentrations. With a suitable tracer, the amount of transfer per unit time can be measured in either direction through a membrane. Intervals of a few minutes are widely used for measurement, but these can be reduced to seconds. Some widely used isotopes, with their half-lives, are: Na^{22} (2.6 years), Na^{24} (14.8 hours), K^{42} (12.4 hours), K^{43} (22.4 hours), Rb^{86} (19.5 days), Br^{82} (34 hours), Cl^{36} (4.4×10^5 years), and P^{32} (14.3 days).

Salt transport is studied in a great variety of objects, some of which are:

- Animal
  - Oxyntic cells of stomach mucosa
  - Nerve axons
  - Microvilli of intestines
  - Kidney nephrons
  - Muscle fibers
  - Epithelium, skin of amphibians and bladder of toads
  - Erythrocytes, single cells
  - Salivary glands
  - Salt glands of birds
  - Mitochondria separated from heart muscle or liver
- Plant
  - Roots of seed plants
  - Yeast, single cells
  - Bacteria, single cells
  - Algae, single cells
  - Salt glands of leaves
  - Mitochondria separated from roots

Most attention has been devoted to transport of Na^+, K^+, and Cl^-. Often, however, H^+ is involved, either as such or as HCO_3^-. Less atten-

tion is turned to Ca^{++} and Mg^{++} except as they modify membrane properties. A special place is accorded the role of phosphate reactions in providing the free energy of transport, but neither its transport nor that of sulfate has been extensively studied.

I will deal chiefly with erythrocytes, frog skin, plant roots, and yeast cells as objects and Na^+, K^+, H^+, Cl^-, and HPO_4^{--} as ions. My experience is restricted to plant roots and yeast cells, but erythrocytes and frog skin better illustrate some of the principles involved in salt transport. General treatments of the animal membrane are to be found in several current books and reviews (Prankerd, 1961; Harris, 1960; Ussing, 1960; and Glynn, 1957).

Salt Transport of Erythrocytes

The red cells in mammals are unnucleated cells without mitochrondria which function as carriers of oxygen through their content of hemoglobin. Their most striking feature of salt transport is change of K^+, Na^+ concentrations in the cell solution relative to the plasma, thus:

ALKALI AND CHLORIDE CONCENTRATIONS IN ERYTHROCYTES AND PLASMA

Species	K^+	Na^+ *Plasma, meq/L*	Cl^-	K^+	Na^+ *Cell Solution, meq/L*	Cl^-	*Reference*
Man	5	155	112	136	19	78	Bernstein, 1954
Cat	5	158	112	8	142	84	Bernstein, 1954
Sheep	5	160	115	64	16	(78)	Bernstein, 1954
				18	84		Kerr, 1937

The deficiency in equivalents between $K^+ + Na^+$, and Cl^- is about half accounted for by HCO_3^- in plasma and one third in the cell solutions. The remaining negative ions are chiefly nondiffusible proteins, which are greater in the cell resulting in a negative potential. Observed osmotic pressures are near zero while the ones calculated on the basis of the ionic concentrations are positive from within.

Water movement in response to osmotic pressure differences is very rapid and probably arises from transport through holes. The generally accorded view for Na^+ and K^+ transport involves both passive diffusion through the membrane and Na^+ transport outward, with accompanying K^+ inward at some point, against an electrochemical potential gradient. External K^+ is required for outward movement of Na^+ (Harris and Maizels, 1951). Chloride ion distributes to maintain neutrality, with a half time of 0.24 seconds for exchange. Transport of Na^+ and K^+ is much slower, being Na^+ 3.1 m M Na^+/liter cells/hour and 2.1 m M K^+/liter cells/hour (Solomon, 1960), corresponding to half times of several days for exchange. This suggests that holes have immobile cations in the walls,

which facilitate passage of negative ions. A positive wall is unexpected, but might arise from choline or ethanolamine groups of the phospholipids which are possibly broken as zwitter ions at pH 7.2 (Glynn, 1957).

Transport of Na^+ in the erythrocyte clearly poses the question: What is the source of entropy for differentiation of Na^+ from K^+? What is the nature of the transducer? The entropy decrease is metabolically derived, for lowering the temperature toward zero, to reduce the metabolic rate, causes the ionic composition of the cellular solution to change markedly toward that of the plasma. Glycolysis is known to occur both through fructose-6-phosphate and ribulose-5-phosphate with lactate as an end product, but the tricarboxylic acid cycle and the respiratory chain for electron transfer to oxygen are lacking. The chief free-energy decrease reactions are conversion of 1,3-diphosphoglycerate to 3-phosphoglycerate and phospho-enol pyruvate to pyruvate, both leading to formation of adenosine triphosphate (ATP) to serve in energy transfer. The order of 70 per cent of the phosphate is present as 2,3-diphosphoglycerate and ATP is about 20 per cent (Bartlett, 1959). The controlling step in glycolysis appears to be utilization of 1,3-diphosphoglycerate from which the 2,3 compound is an off-shoot (Gerlach, *et al.*, 1958).

Attractive hypotheses held for energy coupling are that active transport utilizes energy changes associated with reactions of 1,3-diphosphoglycerate, including formation of ATP or its hydrolysis. The last has received correlative support and is widely upheld. The only other hypothesis to receive support is for energy to be derived from hydrolysis of the lipid α phosphatidic acid in the membrane (Hokin, *et al.*, 1963).

The evidence for an ATPase first came from observations on a peripheral nerve of a crab (Skou, 1960). The enzyme in nerve homogenates requires Mg^{++}, Na^+, and K^+ for maximum activity with the suggested reactions:

$$\text{Enzyme (E)} + \text{ATP} + 2\text{Mg}^{++} + \text{nNa}^+ + \text{mK}^+ \rightleftarrows$$

$$\overset{\text{mK}^+}{\underset{\text{nNa}^+}{\text{E}}}\ \text{Mg}_2\,\text{ATP} \rightleftarrows \overset{\text{mK}^+}{\underset{\text{nNa}^+}{\text{E}}} \sim \text{P} + 2\text{Mg}^+ + \text{ADP}$$

$$\overset{\text{mK}^+}{\underset{\text{nNa}^+}{\text{E}}} \sim \text{P} \rightarrow \text{E} + \text{inorganic phosphate } (\text{P}_i) + \text{nNa}^+ + \text{nK}^+$$

The last reaction is inhibited by strophanthin (and ouabain), a cardiac-active glycoside. When Mg^{++} and Na^+ are present, K^+ activates and then inhibits the enzyme (Fig. 5) as the concentration is increased, finally producing the activity of Mg^{++} alone which is not inhibited by strophanthin. Active alkali transport in the erythrocyte is inhibited by 10^{-5} g/ml of cardiac glycoside (Schatzman, 1953).

A Mg^{++}, Na^+, K^+ activated ATPase activity was found in homogenates of human erythrocyte membranes (Post, *et al.*, 1960; Post and

Albright, 1961). The concentration for half maximal effects of Na^+, K^+, NH_4^+ and ouabain are the same for transport of intact erythrocytes and enhanceable ATPase activity of broken membranes, but the total activation is not great. Similar ATPase activity has now been shown for membranes of cells from nerves, liver, intestine, heart muscle, brain, and kidney. An asymmetry has been reported in the activation of the ATPase, with Na^+ being more effective on the inside and K^+ on the outside of the erythrocyte membrane (Whittam, 1962). Such asymmetry is suggestive of a way in which Na^+ and K^+ could be transported in opposite directions.

Specie and genetic differences for Na^+ and K^+ distributions between plasma and erythrocytes are very evident in cat, contrasted with man

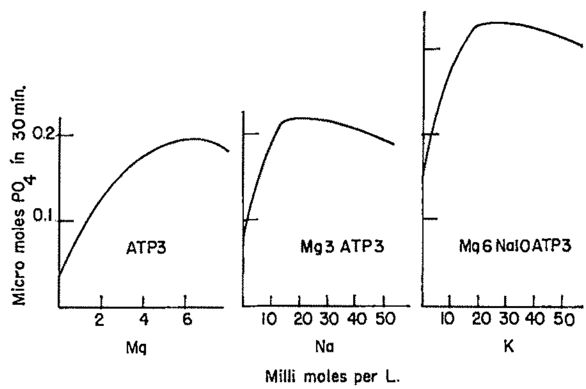

FIG. 5. Activation of crab nerves ATPase by Mg^{++}, Na^+, and K^+ (after Skou, 1960).

(note the preceding table). The cellular contents in the cat approach those of the plasma except as modified by protein content, suggesting that the membrane lacks specificity for transport. This could arise through holes being adequate for transport of all ions (Fig. 2) at a rate appreciably greater than for an energy-coupled exchange. Erythrocytes of sheep are of particular interest in being of both types, depending upon genetics, without reaching the extreme ratio between man and cat. In bladder and kidney tissue, hormonal control of water and salt transport also appear to be on hole size.

Salt Transport Across Frog Skin

The skin is important in the water balance and salt accumulation of amphibians. Transport also takes place across the bladders of toads, and other salientia, used as storage organs. These epithelial membranes are particularly suitable for studies of electrical changes accompanying the transport, in that they can be obtained as sheets and used to separate

solutions of different concentrations. They resemble erythrocytes in having high specificity for differentiating between Na^+ and K^+ while allowing chloride distribution. They differ, however, in the virtual exclusion of K^+ transport across the external surface, in their source of metabolic energy, and in their cellular complexity. Chloride-depleted frogs can take up NaCl from solutions as dilute as 10^{-5} molar and transport it to the plasma where the concentration is 0.1 molar (Krogh, 1937)

If an ionic species passes through a membrane without interacting with other moving particles in the flux (f) the ratio is

$$RT \ln f_i/f_0 = \bar{\mu}_0 - \bar{\mu}_i$$

and

$$f_i/f_0 = (a_0/a_i)(\exp FE/RT) \text{ (Teorell, 1949)}$$

where $\bar{\mu}_0$ and $\bar{\mu}_i$ are the inside and outside electrochemical potentials, and a_0 and a_i are the activities of the ion. When the flux ratios for Cl^- are measured with Cl^{36} across a frog skin, the above equation is satisfied, indicative of Cl^- distribution according to the electrochemical potential gradient, that is, passively toward the positive inside of the membrane. The Na^+ ions, on the other hand, move against both the electropotential and the concentration gradient. The Na^+ flux, accordingly, must be coupled with a free energy source.

If the potential across the frog skin is adjusted to zero by an opposing voltage, and the Na^+ and Cl^- concentration in Ringer solutions are equal on the two sides, NaCl moves across the membrane and the amount transported is equal to the number of coulombs flowing. The Na^+ appears to be actively transported and passively accompanied by the Cl^- to preserve electrical neutrality (Ussing, 1960). How closely does this transport resemble that of erythrocytes? The difference is in K^+ movement, which is absent across the entire skin even when the outside K^+ concentration is half that of Na^+. The active Na^+ transport is very low, however, if a K^+ free inner solution is used (Huf and Wills, 1953). The frog skin in contact with SO_4^{--} instead of Cl^- containing solutions, or when poisoned with Cu^{++}, which prevents transport of Cl^-, acts as a sodium-reversible electrode with respect to Na^+ in the external solution, and as a K^+ electrode with K^+ in the inner solution (Koefoed-Johnsen and Ussing, 1958). The frog skin is a complex array of cells and the potential difference is a sum of several components. When the skin is probed with microelectrodes several potential levels are evident (Engbaek and Hoshiko, 1957).

These findings with regard to potentials across the frog skin are in accord with a model suggested by Koefoed-Johnsen and Ussing (1958) (Fig. 6). According to it, Na^+ and Cl^- passively diffuse through an outer membrane of an epithelial cell. At the inner membrane, an actively driven exchange of Na^+ to the inside and K^+ from the inside takes place as in

the erythrocyte. The K^+ ion passively diffuses back from the epithelial cell to the inner fluid, establishing a steady state. In this regard, a distinctive feature of the frog skin is the impermeable nature of the outer membrane to K^+ while being permeable to Na^+. The potential across the skin is considered to be the sum of a Na^+ diffusion potential at the outer and of K^+ at the inner membrane.

This attractive model is brought under question by observations on the closely similar toad bladder (Essig and Leaf, 1963). The essential facts shown by the frog skin are displayed by the toad bladder. When Na^+ is replaced by choline$^+$ on the side from which the transport

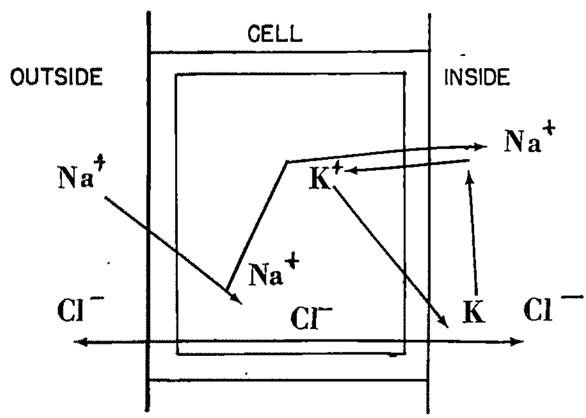

FIG. 6. A concept for transport of NaCl across an epithelial cell of a frog skin (after Koefoed-Johnsen and Ussing, 1958).

takes place (the inside or urinary side of the bladder), however, the exchange of K^+ into the cells of the membrane as measured with K^{42} is unaffected even though the Na^+ transport is decreased by 85 per cent. In other words, the exchange across the particular membrane does not appear to be one of Na^+ for K^+. Rather, the absence of K^+ on the outside of the bladder affects the Na^+ transport at the limiting membrane. Again, the essential question is in the high degree of differentiation between Na^+ and K^+ transport by this membrane. Frazier and Leaf (1963) state "... that the serosal potential step arises from the operation of an 'electrogenic' sodium pump at this surface of the cell, the pump causing the transfer of a sodium ion from the cell interior to the serosal medium without a simultaneous and obligate transfer of an ion in the same direction or a different cation species in the opposite direction as an integral part of the transport step." Electron transport to oxygen in the respiratory chain is "electrogenic."

The Na^+ transport of the frog skin is dependent upon oxygen supply

but only slowly drops to zero under anaerobiosis. The oxygen consumption of the frog skin membrane decreases in the absence of Na^+ in the external solution (Zerahn, 1956) and, if the change alone is considered as involved, about four to five Na^+ are transported per oxygen molecule consumed (Leaf and Renshaw, 1957). Sodium ion transport is inhibited by low concentration of 2,4-dinitrophenol, apparently through uncoupling of oxidative phosphorylation of the respiratory chain. The cardiac glycoside strophanthin, too, is effective as an inhibitor as also are bromoacetamide and other inhibitors of triosephosphate dehydrogenase action in glycolysis. The coupling to free energy change associated with ATP formation is supported and the strophanthin inhibition is suggestive of hydrolysis of ATP being involved. The results obtained with inhibitors, however, are not very restrictive but rather illustrate the difficulty of establishing a specific reaction as affording an energy coupling in contrast to energy supply becoming generally rate-limiting.

Salt Transport in Yeast

Rejuvenation of apparently settled questions as for salt transport across salientia epithelial membranes is also the situation with cells of Baker's yeast. Salt transport in these cells has the apparently inviting simplicity of the erythrocyte for establishing the essential facts (Rothstein, 1956). The cells live and multiply both aerobically and anaerobically, depending in the one condition on oxidative phosphorylation coupled with electron transfer to oxygen as well as glycolysis, and in the other to glycolysis. Salt transport in yeast has been interpreted as showing some of the metabolically linked Na^+, K^+ exchange features of erythrocytes and the H^+ transport of the oxnytic cells of the stomach mucosa (Conway, 1954).

When yeast cells are placed in KCl solutions containing several per cent glucose, they take up K^+ and the medium becomes acid. The process, whatever its nature, is one in which conservation of charge is effected by exchange of K^+ and H^+, the H^+ being metabolically generated (Conway and O'Malley, 1946). If yeast is first grown in the presence of sodium citrate, which results in cells having high sodium ion content, and then placed in KCl solution, an exchange of K^+ and Na^+ takes place (Conway, 1954). The initial rates of these exchange processes are inhibited by 2,4-dinitrophenol and, in the case of K^+, Na^+, by anaerobiosis. These effects of inhibitors seemingly indicate a predominantly aerobic process for rates of Na^+, K^+ exchange, but they really arise from changes in permeability and in generation of acid.

The impression was held that alkali ion uptake by yeast requires glucose except possibly in the presence of citrate which is not utilized.

The uptake rate of Rb^+ at pH 9 was observed, however, to be the same for yeast in the presence of glucose, after citrate incubation, and after starving and washing with 0.001 *N* HCl (Leggett, *et al.*, 1962) thus

(RbCl) Molar	*Uptake in 10^{-9} moles/mg/min* Starved HCl Washed	*Glucose Incubation*	*Sodium Citrate Incubation*
1×10^{-2}	9.0	9.5	9.8
1×10^{-3}	7.7	7.4	6.7
1×10^{-4}	5.3	4.7	5.3
1×10^{-5}	1.3	0.9	1.2

The common feature of the several procedures is removal of calcium. Calcium salts at 10^{-3} molar inhibited the initial rate of uptake of acid-washed yeast by about 90 per cent, but do not alter the final equilibrium. These and other findings suggested that the previous observations on yeast had been affected to various degrees by Ca^{++} concentration as an uncontrolled variable and failure to approach equilibrium. Measurements on acid-washed yeast show that Rb^+ uptake strictly follows an exchange process with H^+ (Leggett, *et al.*, 1962). Sodium yeast could as readily be prepared by washing several times with 10^{-2} molar NaCl or sodium citrate solutions instead of incubating in the presence of citrate.

Cells of all types are observed to have a volume into which ions readily enter and from which they can be removed by exchange. Previously, yeast cells were not considered to take up Cl^-. When the uptake of Cl^- and other anions was examined carefully, it was found that, at 10^{-2} molar, the nonmetabolically active ones rapidly distributed into a volume constituting about 70 per cent of the cellular volume (Leggett and Olsen, 1964), but did not exceed the solution concentration. Yeast cells do not differ essentially from erythrocytes in distribution of chloride except that they are seldom studied in low Ca^{++} solutions as concentrated as 0.15 molar (plasma), They, too, might have various permeabilities for salt, depending upon the source, comparable to those displayed by erythrocytes of sheep.

What is the nature of the 30 per cent of the yeast cellular volume into which chloride does not penetrate? It is found to retain cations incorporated in growth against many hours of exchange or acid washing (Leggett, 1964). The alkali ion content incorporated in growth is only removed after long growth periods, or acid washing for many days, while the predominant component (70 per cent space) is quickly lost. It is likely that the 30 per cent of the ions which are strongly held are components of the cell wall, mitochondria, and endothelial reticulum.

The yeast cell appears to be, in part, a rather permeable sack through which (70 per cent) ions move quite readily and in which many metabolic reactions take place, including glycolysis but not oxidative phosphorylation. The rate of salt transport into the remaining volume is

very slow and its rate has not been measured because of the overwhelming effect of the permeable space. A compartmentalization of yeast of this nature is also suggested by the regulation of glucose utilization under aerobic and anaerobic conditions (the Pasteur effect).

Some Features of Algae

Salt transport into cells of large algae is considered only in similarity and contrast to erythrocytes and salientia epithelia. The cells are favorable objects for study of Na^+, K^+, and Cl^- transport in that electrodes can be inserted to follow potential and conductivity changes and the inner fluids can be modified if desired. The marine algae resemble the erythrocytes in living in a medium of high salt content, 0.6 molar near pH 7, and the fresh water ones are comparable with the skin of the frog. They effect the same striking change in Na^+, K^+ ratios between cellular contents and the medium shown by the erythrocytes and the enhanced concentration across the frog skin. Transport of Na^+ can be against the electrical and the concentration gradients (Blinks, 1955; MacRobbie and Dainty, 1958; Hope and Walker, 1960).

Algae differ from erythrocytes in utilizing photosynthesis as an endogenous energy source rather than glucose in a medium. Their salt transport differs from that of frog skin in that both Na^+ and K^+ move across the membrane. The membranes are polar in the sense of having an inside and an outside but not in the cells having differences between two sides as does the frog skin. The cells have membranes between the cytoplasm and the external medium and the internal large vacuole. The latter are thought to be the more limiting ones for chloride transport and the former for alkali ions.

Salt Absorption by Roots of Seed Plants, Chiefly Phosphate

Roots are the organs through which plants take up water and salts to be transported to the shoots. The cells are the order of 40 μ in cross section and resemble algae in having vacuoles which occupy a considerable fraction of the volume in the older cells. Plants usually do not have a method of salt elimination but rather die if the soil in which they are growing has a high content of soluble salts. Roots are experimentally convenient for study of salt transport in that they can be cut from plants and used in aerated solutions for several hours. They have the drawback of being multicellular with several different types of cells. While electrodes can be inserted into individual cells, roots are much more difficult for such use than are algae.

Salt transport into roots is strongly dependent on oxygen consumption. A striking feature is display of H^+ exchange for ambient cations. Roots do not show the features of Na^+, K^+ exchange so prominently displayed in the objects previously considered and, in fact, are viable

with normal salt transport even at vanishingly low Na^+ contents. They rather require Ca^{++} ions and are generally studied at salt concentrations of less than 10^{-2} molar. Salt accumulation in vacuoles can take place against concentration gradients from 10^{-6} to more than 10^{-2} molar. Most studies have been devoted to initial and steady state rates of salt accumulation since equilibrium is approached only after long times at high concentrations. Equilibrium results, however, are instructive in showing an eventual reversibility of the sorption process.

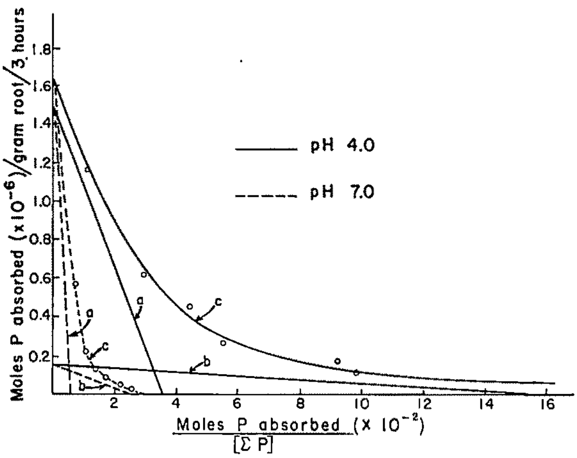

FIG. 7. Composition of OH^- with phosphate absorption by barley roots (after Hagen and Hopkins, 1955).

Attention is first turned to phosphate uptake, which was not considered in the previous objects, because it best shows the nature of the energy dependence of the accumulation process, which, for phosphates, is generally against a very high concentration gradient. It is instructive to consider phosphate uptake according to the Michaelis-Menton formulation of enzyme kinetics. Phosphate (X) of outside concentration $[X]$ is assumed to form an intermediate compound, EX, with a wall or cell constituent, E, which breaks down as follows:

$$E + X_{outer} \underset{k_2}{\overset{k_1}{\rightleftarrows}} EX \xrightarrow{k_3} E' + X_{inside}$$

The rate of the process, V, is

$$V = \frac{-V}{[X]} K_m + V_{max}$$

where V_{max} is the maximum rate of uptake at high concentrations and $K_m = (k_3 + k_2)/k_1$ (Epstein and Hagen, 1952; Mitchell, 1954). The rate of phosphate uptake by barley roots at various concentrations and

two pH values is shown after this formulation in Figure 7 (Hagen and Hopkins, 1955). A single process would give a linear dependence of V on $V/[X]$. The uptake at each pH can be resolved into two lines differing about tenfold in the intercept, V_{max}, on the ordinate axis. As the pH is increased from 4.0 to 7.0 the phosphate uptake at a given concentration decreases greatly but V_{max} values for the two processes remain the same (Fig. 7). This is indicative of some type of competition with phosphate uptake, the competition likely being by OH^-. When this competition is taken into account, it is seen that both $H_2PO_4^-$ and HPO_4^{--} are taken into the cell, with the former far more pronounced at high concentrations.

Inorganic phosphate enters into metabolism chiefly through formation of ATP and, in aerobic organisms, this is principally associated with

S.D. ← succinate → b''; succinate | "starvation"

$$O_2^- \leftarrow a_3'' \leftarrow a'' \leftarrow c_1'' \leftarrow (c'') \leftarrow b'' \leftarrow \text{rfp} \leftarrow \text{DPNH} \leftarrow \beta\text{-hydroxybutyrate} \qquad (11)$$

O_2^- | anaerobiosis; a_3'' | CN^-, HN_3, CO; b'' | antimycin A | naphthoquinone SN 5949 | 3 hydroxy-1-heptylquinoline-N-oxime; rfp | amytal; β-hydroxybutyrate | "starvation"

FIG. 8. The respiratory chain for electron transfer to oxygen (after Chance and Williams, 1956).

electron transfer to oxygen through the cytochromes and other components of the respiratory chain. The detailed manner of this oxidative phosphorylation is still under discussion (Lehninger, 1962; Chance and Williams, 1956). It suffices here that formation of ATP takes place at three of the oxidation reduction steps (Fig. 8), the functioning of each of which is established by action of suitable inhibitors or coupling to various reductants (Hagen, *et al.*, 1957). One of these is the barbiturate, nembutal, which prevents electron transfer from reduced diphosphopyridine nucleotide to flavoprotein. Its effect on phosphate uptake is shown in Figure 9, to be a decrease in the intercept on the ordinate, V_{max}, for one of the processes. In a similar way, use of succinate and various inhibitors establishes another place of phosphate uptake as coupled to oxidation of cytochrome-b by-c. Uncouplers of oxidative phosphorylation such as 2,4-dinitrophenol, at 5×10^{-6} molar, strongly inhibit both of the recognized steps of phosphate uptake by barley roots.

The respiratory chain is carried by mitochondria which can be separated from animal or plant tissue. It appears, therefore, that the rate-limiting steps of phosphate uptake in barley roots take place in mitochondria. This is surely not a universal phenomenon for all objects, as the rate-limiting step of phosphate uptake in the erythrocyte, which does not have mitochondria or respiratory chains, is associated with ATP formation by triosephosphate dehydrogenase action. In yeast,

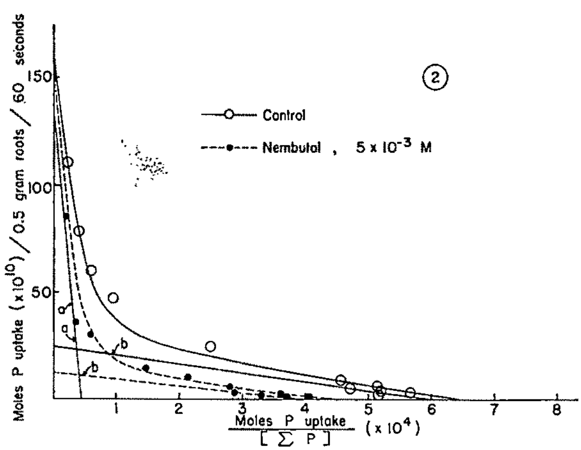

FIG. 9. Inhibition of phosphate uptake by barley roots by the barbiturate nembutal (after Hagen *et al*, 1957).

also, the phosphate uptake is chiefly associated with triosephosphate dehydrogenase action, as shown by its presence under anaerobiosis and by the action of inhibitors, even though mitochondria are present and aerobically functional. A small component of aerobic phosphate uptake by yeast, effective at low phosphate concentrations, is associated with the respiratory chain. The mitochondria isolated from barley roots show oxidative phosphorylation, as do other mitochondria, and have the same properties toward inhibitors and substrates for phosphate uptake as do barley roots (Jackson, *et al.* 1962).

It is difficult to picture the manner in which phosphate uptake by mitochondria could be rate-limiting for phosphate uptake from the external medium by barley roots. Inorganic phosphate concentrations in the cytoplasm around the mitochondria in living cells can greatly exceed that of the ambient medium and the electrical potential is probably negative. The conclusion is forced from this consideration that the mitochondria are in quite effective contact with parts of the endothelial reticulum and outer walls of the cells. They are known to be moving rapidly in cells and, in vacuolated plant cells, the cytoplasm is quite

thin. In any case, the strong aerobic dependence of salt uptake of roots supports oxidative phosphorylation as the source of free energy.

Mitochondria in oxyntic cells of the stomach, kidney structures, membranes, and in muscle for which salt transport is a major function are prominent cellular constituents and are closely associated with the functional membranes which, by location alone, is further evidence for an essential part in the transport process. This association for ions other than phosphate could be as a source of ATP if an ATPase action is basic for ionic transport across membranes. Supporting evidence is poor for such action in barley roots. The rate of phosphate release from ATP at 5×10^{-4} molar by intact roots and by fiber, cell walls, mito-

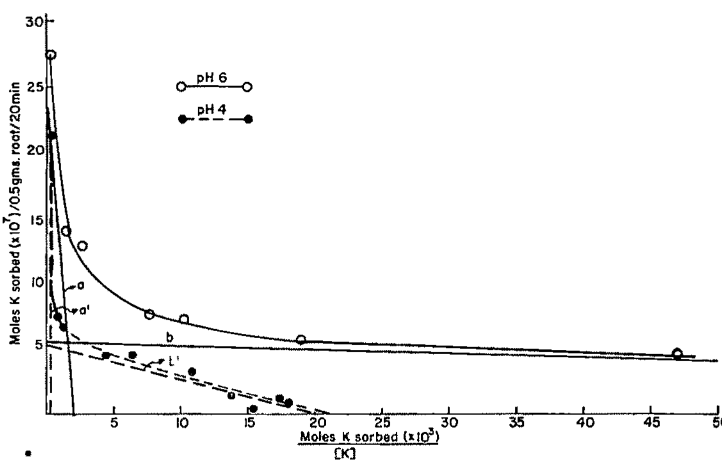

FIG. 10. Competition of H^+ and K^+ absorption by barley roots (after Fried and Noggle, 1958).

chondria, and microsomes is only a few per cent of the rate of K^+ and Na^+ transport by the roots in 1×10^{-3} molar chloride solutions. Activation of ATPase action by K^+, Na^+, Mg^{++} and inhibition by ouabain is less than 20 per cent for cell walls and mitochondria.

A component of respiration of root tissue is correlated with salt absorption (Lundegårdh and Burström, 1935). This component, which is designated as anion respiration, is apparent when salt concentration is increased and is sensitive to inhibition by cyanide. Study of the cyanide inhibition, in detail, on salt absorption indicated that 3 to 4 moles of a uni-univalent salt are absorbed for each molecule of oxygen consumed (Robertson and Wilkins, 1948). Similar results were obtained on salt transport by isolated frog skin (Zerahn, 1956). Reported values of salt absorption exceeding 5 moles for each oxygen molecule probably

arise from measuring the salt and O_2 as diffusion against background values (Leaf and Renshaw, 1957). A decrease in oxygen consumption, in the absence of Na^+ in the bathing medium, was noted for the toad bladder. The toad bladder also transports NaCl under anaerobic conditions, the observed value being 1.7 Na^+ ion per lactate molecule formed, with about 20 per cent of the glycolysis being reckoned as involved. The transport should accordingly, be considered 8.5 Na^+ (Leaf, *et al.*,

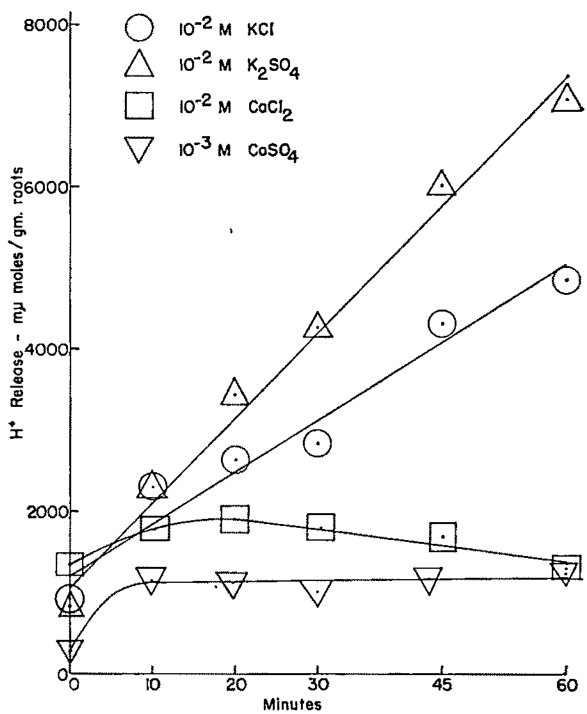

FIG. 11. Release of H^+ accompanying absorption of various salts by plant roots. Absorption of $CaCl_2$ is accompanied by release of OH^- (after Jackson and Adams, 1963).

1959). Sodium ion uptake under very low oxygen pressures has also been observed in plant roots.

Salt Absorption by Roots of Seed Plants. Exchange with H^+ and OH^-

Roots appear to absorb anions and cations independently in exchange for H^+ and OH^- (Jacobson, *et al.*, 1950). It is also held that the independence is only apparent, resulting rather from compensatory involvement of H^+ ions and electrons arising from action of the respiratory chain (Burström, 1951; Lundegårdh, 1954). The situation is similar to

transport across the frog skin where NaCl apparently moves alone, but K^+ is involved.

Absorption of Na^+, K^+, and Rb^+ by barley roots depends on salt concentrations in much the manner of phosphate. All are competitively inhibited, however, by H^+ indicative of an exchange process (Fig. 9) (Fried and Noggle, 1958). Absorption of Na^+ and Rb^+ is competitively inhibited by K^+, but Na^+ interacts with K^+ only indirectly when present in very large excess.

The rate of K^+ absorption from sulfate, phosphate, and chloride solutions is closely the same, even though the rate of chloride absorption exceeds that of sulfate by 12-fold at 10^{-2} molar and by 100-fold at 10^{-4} molar. Absorption of K^+ from K_2SO_4 solutions is chiefly compensated by transfer of H^+ from the root to the solution (Fig. 11) (Jackson and Adams, 1963) to maintain equivalence of charge. In the case of KCl solutions, the H^+ transfer is less because of higher transport of Cl^- than SO_4^{--} by the root. When base release by roots is measured for $CaCl_2$, where transport of Ca^{++} is low relative to Cl^-, the roots are found to release OH^-. The controlling process for necessary charge balance is one of compensation, by H^+ or OH^- release, by the root as required. This acid base exchange is a dominant feature of salt absorption by higher plants rather than the Na^+, K^+ exchange shown by erythrocytes and many other membranes of animal cells. An interesting model of an acid base exchange system mediated by guaiacol was worked out by Osterhout and Stanley (1932).

Mitochondria and Salt Transport

Mitochondria are intensively studied as the principle energy-coupling system in higher organisms (Chance and Williams, 1956; Green and Hatefi, 1961; Lehninger, 1963; Green, 1964). They are considered here only with respect to salt transport which is concomitant with their major function (Brierley, *et al.*, 1963). They can be separated for study or their action can be inferred in intact organisms. They possibly participate in salt transport across cellular membranes through rate-limiting processes in their double membranes.

Several suggestions have been made about their manner of salt transport. One of these was by Lundegårdh, to quote (Lundegårdh, 1945, 1954): "If the cytochrome system is part of a membrane structure with the oxidase facing the medium and cytochrome-b facing the place of accumulation, conditions will be fulfilled for an active absorption of salts from the medium into the cell." He considered anions to pass from cytochrome-a to cytochrome-b opposite to electron flow. Green and his associates sought to differentiate between Mg^{++} and Ca^{++} accumulation "directly by the oxidative phosphorylation system or by reactions

contingent on the production of ATP." They found that oligomyacin which inhibits ATP formation had little effect on Ca^{++} accumulation in the presence of succinate and reasoned that the accumulation is supported by an intermediate (high energy carrier) between the oxidation-reduction site and ATP formation in oxidative phosphorylation. Calcium accumulation is involved (pH 7.5) (Ebashi and Lipmann, 1963) with calcium phosphate precipitation as a sort of trapping system. In Mitchell's view (Mitchell, 1961), formation of ATP or the reverse ATPase action, at the mitochondrial membrane is connected with an asymmetrical hydrolytic reaction such that OH^- equilibrates inwardly and H^+ outwardly. These concepts all have the common feature of ionic transport with which at least H^+ is associated in an internal cycling and OH^- can also play a part. At present asymmetry of transport is noted rather than explained.

Résumé

The present knowledge of salt transport across living membranes leaves many questions unanswered about the processes involved. Transport against an electrochemical potential difference leading to appreciably enhanced salt concentrations and differentiation between ions within cells is well demonstrated both for animal and plant tissues. The associated reactions supplying the free-energy decrease for the transport appear to be close to the formation or hydrolysis of adenosine triphosphate (ATP). Hydrolysis of ATP is considered to be important for membranes across which NaCl is transported or exchange of Na^+ or K^+ occurs. Ions of Na^+, K^+, and Mg^{++} promote the activity of ATPases in membranes of this type. Heart-active glycosides and Ca^{++} inhibit both the transport and the ATPase activity. Anaerobiosis is only slowly inhibitory on frog skin and 2,4-dinitrophenol, malonate, azide, and carbon monoxide have little effect on erythrocytes.

Transport in many other membranes functioning under aerobic conditions is closely associated with oxidative phosphorylation. This transport involves H^+ and OH^- exchanges, does not require Na^+, is somewhat enhanced by Ca^{++}, is not inhibited by SO_4^{--} or heart glycosides, but is markedly inhibited by 1×10^{-5} molar 2,4-dinitrophenol and by other inhibitors of oxidative phosphorylation. Accumulation of Ca^{++} in mitochondria is inhibited by 1×10^{-5} molar oligomyacin indicative of a phosphate intermediate of ATP synthesis being involved.

The difficulties of understanding the detailed manner of asymmetrical salt transport across membranes are still formidable. While the properties of artificial membranes with holes are far better understood than are living membranes, extension of the knowledge to living systems is tenuous, even to the surety of holes being present. A model has been suggested and later revised for salt accumulation depending on two

membranes, one selective for anions and the other for cations, with acid addition as the free-energy source and a special type of mixing (Sollner, 1955). Acid generation, including HCO_3^- and formation or hydrolysis of ATP, is a feature of many parts of living systems closely involved in salt transport. The asymmetry might result from acid generation within the cell. Salt exchange in Baker's yeast in which acid is formed internally is possibly of this type.

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AMERICAN SCIENTIST 52, 1964

THE LACUSTRINE MICROCOSM RECONSIDERED

By G. E. HUTCHINSON

THE great intellectual fascination of limnology lies in the comparative study of a great number of systems, each having some resemblance to the others and also many differences. Such a point of view presupposes that each lake can in fact be treated as at least a partly isolated system.

Today [1] I want to begin by considering two rather different approaches implicit in such treatment, partly in the work of Birge and Juday during the time when they were making Lake Mendota famous throughout the scientific world, and partly in the earlier work of S. A. Forbes from whom my title is of course derived.

Birge's mature point of view is expressed in his concept of the heat budget [2], which, though derived from ideas of Forel and others, represented a highly original and important contribution, because it first called attention to the lake as a natural system with an input and an output. This point of view has tended to underlie most of what has been done in lake chemistry and in the study of primary productivity during the past three or four decades. Such a way of thinking, in which the lake is considered, in the jargon of the moment, as a black box, has been called elsewhere [3] the *hololological* approach. It has been extremely fertile, but, since water is transparent, the black box is too restrictive an analogy. The time has perhaps come for further development of the antithetical *merological* approach, in which we discourse on the parts of the system and try to build up the whole from them. This is what Forbes was trying to do in his classical lecture on "The Lake as a Microcosm" [4].

It is desirable to think for a moment about certain scale effects characterizing the lacustrine microcosm when viewed by a human observer. If we suppose that an organism reproduces about once every week for the warmer half of the year and on an average about once every month in the cooler half, it will have about thirty generations a year. This corresponds in time to about a millenium of human generations, and considerably longer for those of forest trees. In the case of the latter, we should expect in thirty generations some secular climatic change to be apparent. We should not expect in a tree the seeds or resting stages to remain viable while thirty generations passed, and in the larger animals no such stages exist. The year of a cladoceran or a chrysomonad, in both of which groups rapid reproduction may alternate with the formation of resting stages, is thus in some ways comparable to a large

segment of postglacial time, though in other ways the comparison either to several millenia, or to a year in the life of a human being or tree, is definitely misleading. Another peculiar scale effect is that, in passing from the surface to the bottom of a stratified lake in summer, we can easily traverse in 10–20 m. a range of physical and chemical conditions as great or greater than would be encountered in climbing up a hundred times that vertical range on a mountain.

I would also emphasize how fantastically complicated the lacustrine microcosm is likely to be. There is probably no almost complete list of species of animals and plants available for any lake, but it would seem likely from the several hundred species of diatoms and of insects [5] known from certain lakes that a species list of the order of a thousand entries may be not unusual. This probably means that in the course of a season at least a thousand somewhat different ecological niches may for a time be recognizable. Most of this diversity is associated with the shallow marginal waters in which the bottom can form a solid substratum for attached aquatic plants.

Simpler situations in a lake are probably provided by the plankton, though it soon appears that they are not particularly simple and that we cannot regard the plankton, excluding the rest of the community, as an entirely satisfactory entity. We begin with the variously named and deductively respectable principle that two co-occurring organisms cannot form equilibrium populations in the same niche. In the phytoplankton we immediately meet the paradoxical situation of an enormously complicated association of phototrophic species all living together under conditions that do not seem to permit much niche specialization.

It is possible that the permanent and apparently almost monospecific *Anacystis* blooms recorded [6] under some conditions in tropical waters, notably temple tanks of South India, may represent a monospecific equilibrium of the kind to be expected from theory. Much more often, what I have elsewhere termed the paradox of the plankton intrudes itself. It is to be noticed that the paradox of a multispecific phototrophic phytoplankton only arises if we assume a closed system, providing a single niche, with enough time to permit the achievement of equilibrium. In general in a lake, we do not have a single niche system that is closed. The epilimnion if reasonably turbulent may approach a single niche system, but introductions from the littoral benthos are always possible. Moreover, there is no rule about the speed at which competitive exclusion excludes. As Hardin [7] has pointed out, in the theory all that is needed is an axiom which states that no two natural objects, or classes of objects, are ever exactly alike. What then happens is that under constant conditions one class, or population of reproducing objects, finally displaces the others. If the conditions are continually changing, the favored species might also change. This is what usually seems to be

happening, but it must not be forgotten that a multispecific system never in equilibrium would be expected to suffer continual random extinctions and, if not quite closed, random reintroductions also, and should therefore drift in specific composition, probably more than is indicated by palaeolimnological data.

It is possible that, in a lake, random extinction is primarily a danger for the rarer species which are never likely to be observed. In a square basin 100 m. across and one meter deep we should have 10^6 organisms so common that one occurred per cubic meter, 10^9 of those occurring a thousand times more often at a rate of one per liter, and 10^{12} of those with one individual in the average cubic centimeter. If we are considering ordinary phytoplankton organisms, the first organism would be far too rare ever to find by ordinary techniques even though the population before us numbered a million.

I am now inclined to think that a large part of the diversity of the phytoplankton is in fact due to a failure ever to attain equilibrium so that the direction of competition is continually reversed by environmental changes, as suggested many years ago, moderated in two ways which insure that competitive exclusion does not continually and irreversibly remove bits of the association. The first moderating influence is the speed at which exclusion occurs. In spite of the ultimate validity of Hardin's axiom of inequality, Riley [8] feels that a sort of asymptotic approach to more or less equal adaptation is not unexpected in the phytoplankton. If we suppose two species S_1 and S_2, such that in niche $\mathbf{N}_1$, S_1 displaces S_2, and in niche $\mathbf{N}_2$, S_2 displaces S_1, if seasonal environmental changes occur, so that at first only $\mathbf{N}_1$ and then $\mathbf{N}_2$ are available. If competition went fast compared with the rate of environmental change, S_1 would be eliminated and would not be available for a new cycle, but if the two species were almost equally efficient over a wide range of environmental variables, competitive exclusion would be a slow process. Both species then might oscillate in varying numbers, but persist almost indefinitely.

The second way of moderating the tendency to random extinction is the provision of resting stages, so that if S_1 is eliminated completely as an active competitor in the plankton, when $\mathbf{N}_1$ gives place to $\mathbf{N}_2$, later next season when the reverse change occurs, resting stages of S_1 can recolonize the environment now again providing niche $\mathbf{N}_1$. In practice, any plankter that really disappears and reappears rather than becomes alternatively rare or very common must have some such stages. Annual macrophytes and many small animals also have such stages as seeds, eggs, pupae, and the like. Perennials, moreover, may hibernate in ways that take the individual out of competition. In the diatoms in many of which resting zygospores are still unknown, it is possibly relatively unmodified littoral or shallow water benthic individuals that are involved

in tiding the planktonic populations over competitively unfavorable conditions. In *Melosira*, Lund's beautiful work [9] shows how a relatively heavy diatom rests on the bottom for very long periods in a more or less unassimilative form; here there is doubtless some special physiological adaptation, so we are halfway between a species invading the plankton casually with a continuous littoral population and the condition in which morphologically specialized resting stages or cysts are produced. One of the most remarkable results of several recent palaeolimnological studies, notably Nygaard [10] on Store Gribsø and our own work on Lago di Monterosi [11], is the fantastic variety of Chrysophycean cysts recognizable in the sediments, at least, of rather soft water lakes. Resting stages of all sorts are of course particularly prone to occur in freshwater organisms, where they were doubtless developed primarily to promote survival under extreme physical conditions, notably desiccation and freezing. Once developed, they would however clearly be of great value in obviating extinction when conditions changed in favor of a competitor. It is therefore peculiar that Lund finds that planktonic desmids tend to lose such stages.

The diversity of the phytoplankton is clearly of primary importance in producing that of the zooplankton. Given the diversity of the phytoplankton, and some degree of food specificity in the animal forms, no striking paradoxical situation need arise. Moreover, it is clear from all the available work on the seasonal succession of closely allied forms, such as the species of *Daphnia*, from Birge's [12] early studies on Mendota up to the very beautiful and elaborate investigations of Dr. J. L. Brooks and of Dr. Donald W. Tappa at Yale, shortly to be published, that the same sorts of seasonal phenomena that damp competition in plants, also occur in animals.

MacArthur and Levins [13] have recently pointed out that two rather different extreme types of diversity between closely allied sympatric species (i.e., members of a genus or subfamily) are possible.

The two species may be specialized in such a way that they eat slightly different food, but hunt it over the same area. In this case morphological specializations, of which the simplest is a size difference, are to be expected. Probable examples, such as the hairy and downy woodpeckers, easily come to mind.

If the two species eat the same sorts of diversified food, they are likely to differ in the proportions in which they encounter it, and to specialize in habitat preferences without much morphological specialization becoming necessarily involved in feeding activity. MacArthur's own work on the American warblers provides a striking example. The existence of these two general situations has long been known, but MacArthur and Levins provide a good abstract theory of the phenomenon.

Over the whole vertical column in a stratified lake, even if only 10 m. deep, habitat differences are available in summer, at least as great as over the range from the bottom and to the top of a mountain several hundred times that vertical range. In the turbulent epilimnion it is in general hard to develop habitat preferences and, within any layer in which free movement is habitual, size differences may be expected as the simplest specialization increasing diversity, as is the case with copepoda. In view of the extreme vertical variation when we leave a turbulent freely mixed layer, the antithetic habitat difference type of specialization in the plankton is likely to be rather different from what is found terrestrially, involving fairly complete adaptation to very divergent physical factors rather than habitat preferences, though, in two species living together with vertical migration over partly overlapping ranges, we have the lacustrine analogue of birds feeding in different parts of the same tree. The rapid production of a number of generations per year permits a kind of seasonal succession in rotifers and cladocera, though to a less extent in copepoda, that is comparable to that in the phytoplankton. Considerable possibility of avoiding competitive exclusion is thus achieved by slow competition between species that have slightly different optima, and so succeed each other in time. Here the production of resting stages is of the greatest importance. That they are produced at the time of maximum population fits reasonably into this scheme quite independently of adaptation to unfavorable physical situations.

This succession in time may be coupled with size differences and habitat differences, probably producing, in for instance the genus *Polyarthra* [14] where five or six species can be sympatric but not strictly synchronic, a very pronounced niche specificity.

An interesting question arises, namely to what extent sympatric species of a given taxon, say genus or family, not merely have different ecologies, but also have ecologies that, though different, are closer than any would be likely to be to that of a sympatric nonmember of the taxon picked at random.

If we compare a desert assemblage with a limnoplanktonic one, it is obvious that, if the first organism captured in the desert is a beetle, the probability that the next one of another species is also another beetle is higher than that it is a rotifer, and vice versa. It is however rather surprising to find that in Carlin's data [14] four unallied perennial rotifers, including the microphagous sedimenters *Keratella, Notholca,* and *Conochilus* and the selective predator *Asplanchna,* all reacting similarly to an unidentified difference, possibly involving an earlier decline in the late summer bloom of *Oscillatoria,* that distinguished 1940 from the other years of his study.

The relatively small development that has been possible in the study of the interrelationships of the plankton since Forbes [4] wrote in 1887

and Birge [12] in 1898, and of which some examples have just been given, has been due to a very large amount of work both in field observations, in very meticulous taxonomy and in ecological theory. In the other parts of the lacustrine community the problems are more difficult though their solutions would have great fascination, as will be apparent from a single example. If we examine the lasion, "Aufwuchs" or fouling community of freshwaters, we find a variety of filamentous algae and diatoms with an associated fauna ordinarily of small motile forms. The biomass of the animals is doubtless ordinarily much less than that of plants, and on a surface near the bottom of the euphotic zone organisms will tend to be scarce. There may be a few sponges and bryozoa but they are not conspicuously important. In the sea, the parallel community, though largely algal in the tidal range, consists at most levels of an astonishing mass and variety of sessile animals, sponges, molluscs such as *Mytilus*, numerous hydroids, bryozoa, and tunicates. The difference is presumably due to the lack of pelagic larvae other than copepodan nauplii in freshwater. The only exceptions are a very few molluscs of far from worldwide distribution, notably *Driessena* and to a less extent *Corbicula*, the larval colonies of phylactolaematous bryozoa, hardly ever noted in open water, perhaps a few transitory planula larvae (*Cordylophora*, *Limnocnida*, *Craspedacusta*) and the free swimming larvae of Trematodes which show odd diversity in behavior when allied species are compared, but which presumably do not enter into competition with other plankters. This is a very poor showing compared to the dozen phyla likely to be found in any series of marine neritic plankton samples. This difference has received various explanations, the most reasonable, essentially due to Needham [15], probably being the difficulty that a small larval animal in freshwater, feeding on plant cells low in sodium and chloride, would have in acquiring enough salt before it could develop salt absorbing organs. In a certain sense the adult animals of the marine littoral benthos, not all sessile nor all microphagous, are the resting stages removed from competition at least in the open water plankton. We see in this type of relation the rather large scale example of the sort of interaction which fascinated Forbes. It is hard, in reading Forbes on The Lake as a Microcosm, to prevent the mind drifting back to the tangled bank of the last chapter of Darwin's *Origin of Species* [16], and from there it is permissible, at least in 1964, to look back still further to that other "bank where the wild thyme blows" [17]. Both Forbes and Darwin realize struggle but see that it has produced harmony. Today perhaps we can see just a little more. The harmony clearly involves great diversity, and we now know, in the entire range from subatomic particles to human artifacts, that every level is surprisingly diverse. We cannot say whether this is a significant property of the universe; without the model of a less diverse universe, a

legitimate but fortunately unrealized alternative, we cannot understand the problem. We can, however, feel the possibility of something important here, appreciate the diversity, and learn to treat it properly [18].

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16. The concluding paragraphs of Forbes' essay and of the *Origin of Species* may be profitably compared:

> Have these facts and ideas, derived from a study of our aquatic microcosm, any general application on a higher plane? We have here an example of the triumphant beneficence of the laws of life applied to conditions seemingly the most unfavorable possible for any mutually helpful adjustment. In this lake, where competitions are fierce and continuous beyond any parallel in the worst periods of human history; where they take hold, not on goods of life merely, but always upon life itself; where mercy and charity and sympathy and magnaminity and all the virtues are utterly unknown; where robbery and murder and the deadly tyranny of strength over weakness are the unvarying rule; where what we call wrong-doing is always triumphant, and what we call goodness would be immediately fatal to its possessor—even here, out of these hard conditions, an order has been evolved which is the best conceivable without a total change in the conditions themselves; an equilibrium has been reached and is steadily maintained that actually accomplishes for all the parties involved

the greatest good which the circumstances will at all permit. In a system where life is the universal good, but the destruction of life the well-nigh universal occupation, an order has spontaneously arisen which constantly tends to maintain life at the highest limit—a limit far higher, in fact, with respect to both quality and quantity, than would be possible in the absence of this destructive conflict. Is there not, in this reflection, solid ground for a belief in the final beneficence of the laws of organic nature? If the system of life is such that a harmonious balance of conflicting interests has been reached where every element is either hostile or indifferent to every other, may we not trust much to the outcome where, as in human affairs, the spontaneous adjustments of nature are aided by intelligent effort, by sympathy, and by self-sacrifice? (Forbes.)

It is interesting to contemplate a tangled bank, clothed with many plants of many kinds, with birds singing on the bushes, with various insects flitting about, and with worms crawling through the damp earth, and to reflect that these elaborately constructed forms, so different from each other, and dependent upon each other in so complex a manner, have all been produced by laws acting around us. These laws, taken in the largest sense, being Growth with Reproduction; Inheritance which is almost implied by reproduction; Variability from the indirect and direct action of the conditions of life, and from use and disuse: a Ratio of Increase so high as to lead to a Struggle for Life, and as a consequence to Natural Selection, entailing Divergence of Character and the Extinction of less-improved forms. Thus, from the war of nature, from famine and death, the most exalted object which we are capable of conceiving, namely, the production of the higher animals, directly follows. There is grandeur in this view of life, with its several powers, having been originally breathed by the Creator into a few forms or into one; and that, whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been, and are being evolved. (Darwin.)

17. I know a bank where the wild thyme blows,
Where oxlips and the nodding violet grows;
Quite over-canopied with luscious woodbine
With sweet muskroses, and with eglantine
There sleeps Titania sometime of the night,
Lull'd in these flowers with dances and delight;
And there the snake throws her enamell'd skin,
Weed wide enough to wrap a fairy in.
—A Midsummer's Night's Dream
II. i. 249–256

18. The evening program ended with a performance of Mozart's Quartet in G Minor for piano and strings, K. 478.

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THE STRATEGY OF EVOLUTION*

By LAWRENCE B. SLOBODKIN

There is an embarrassing lack of surprises in certain aspects of evolutionary theory. For example, if a bird has only seawater to drink, it is not surprising, given our knowledge of birds in general, that some mechanism must exist for eliminating salt if the bird is to survive. It is not possible to state *a priori* what the mechanism is for any particular bird. It is therefore intellectually pleasing that gulls excrete strong brine through a nasal gland (Schmidt-Nielsen, 1960). The novelty of the arrangement provides the same kind of intellectual satisfaction in the twentieth century that I imagine the information in the Bridgewater treatises provided in the nineteenth (Whewell, 1833), or Henderson's *Fitness of the Environment* (1913) to the teachers of our teachers. Nevertheless, physiological and even behavioral adaptations, no matter how apt, seem only what ought to be expected from our knowledge of natural selection, survival of the fittest, and other catch words.

In the literature of biology, discussions of evolution consist of either historical statements, whether phylogenetic or genetic, and of physiological or morphological descriptions. Despite this reticence on the part of biologists, an enormous amount of bad philosophy, psychology, and, on occasion, political theory, has been initiated by a consideration of evolutionary phenomena. There is apparently an almost irresistible temptation to try to find some point, purpose, or goal in the evolutionary process. In some sense, the narrowly biological discussions of evolution are unsatisfying, so that para-biologists feel called upon to provide a theatrically acceptable finale.

Even among biologists, goal-oriented evolutionary statements are made in private conversation. We tend to speak in generally teleological terms as if the confrontation between organisms and their environment were some kind of game in which the successful organism has somehow outwitted a powerful but not necessarily malicious opponent. This manner of speaking does not do violence to normal standards of mechanistic science since it is an axiom of mechanistic biology that, in fact or in principle, any teleological statement in biology is replaceable by a suitably non-teleological one. The non-teleological replacement statement may always be made of two parts, first, a physiological or biochemical description of the events occurring in the organism confronted with the situation of interest, second, a statement about the effect of these events on the probability of survival of the organism.

The process of taking organisms from one environment and placing

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them in another can be thought of, from the quasi-teleological viewpoint of a game analogy, as confronting them with a problem to solve. More usually, the situation would be described in physiological terms. The actual responses of the organisms to this new situation are quite independent of the observer's viewpoint, as are the data recorded from the experiment. The conclusions drawn from the experiment, the experiments undertaken, and the kind of data actually recorded are, however, very much dependent on the observer's viewpoint.

I will present one set of experimental examples and will interpret these examples as if they constituted a kind of game between the organisms and their environment. I will then attempt to make statements about the game analogy in evolutionary theory and about the "purpose" of evolution.

I accept as an initial axiom (as did Darwin and Wallace) that, barring drastic physical changes or the introduction of new species, the number and kind of animals found in any region of the world tend to remain essentially constant from year to year.

It is also a valid experimental platitude that if a few fertile organisms are taken from either the field or a stock culture and placed in a suitably salubrious but spatially confined environment it is vanishingly unlikely that their general physiological state will not change. In particular, their abundance in the new environment will come to some new level which will thereafter characterize the experimental container. We will refer to this relatively constant population as the "steady state population."

The fact of introducing them to a new container is a violent alteration of the environment of the animals in the inoculum. If the inoculum is sufficiently small, the number of animals will increase. When careful examination of animals in increasing populations has been made it has been found that the animals are growing more rapidly as individuals, are reproducing more rapidly, have a higher fat content, and by most reasonable criteria are healthier individuals than animals of the same kind taken from a steady state population in the same environment (Smith, 1963). The degree and kind of physiological changes that occur in the individual animals of a population during the transition period between the inoculation and the attainment of steady state vary from species to species but they always do occur. This is tautological, since the steady state population can be defined by an identity of birth and death rates while death rates are significantly lower than birth rates in an increasing population. We conclude that there is generally an evolutionary advantage to having a relatively larger number of less healthy animals than to having a smaller number of more healthy ones. There is some balance struck for any particular species in any particular environment between optimal physiological condition of the individual and optimal

abundance. The mechanism of achieving this balance differs from species to species.

Recent experimental studies in my laboratory permit a tentative analysis of the adaptive significance of this balance in two species of hydra. The hydra are morphologically simple animals with no hard skeletal structure at all. Essentially, they consist of a hollow bag of tissue with tentacles growing around the lip of the bag. The tissue layers are relatively thin. The body wall thickness, as well as the size and shape of the animals, can be altered within fairly wide limits by differential contraction and relaxation of muscle-like cells. In still water, for example, the tentacles may be extended as fine filaments 20 cm or longer while the same animal when shaken will shrink to a mound not more than 3 mm across. This drastic alteration in the morphology of a single individual is fairly common among the soft-bodied coelenterates.

If either green or brown hydra are inoculated in small numbers into an experimental container with an abundant but constant daily food ration, they will initially grow larger as individuals and produce buds. As the number of actively feeding mouths increases, the food available for each mouth is reduced. As this reduction occurs, the budding rate falls off and eventually ceases and the body size of the individual animals diminishes. *Hydra oligactis*, *Hydra littoralis*, *Hydra pseudoligactis* (the brown hydra in my laboratory) or *Hydra viridis* and *Chlorohydra viridissima* (green hydra with symbiotic algae embedded in their body cells) all overlap broadly in their size ranges so that any hydra in any of these species can be brought to the same body size as any other by appropriate feeding. In the process of population growth, each population will increase numerically and the animals in it will come to a fixed body size, considerably smaller than their genetic potential. Griffing (personal communication) has shown that in *H. oligactis* and *H. pseudoligactis* there is a direct relation between body weight, budding rate, and feeding rate. This seems valid for all hydra. The size of animals in a steady state population is therefore that size at which the budding rate is just sufficient to compensate for the mortality rate.

While the precise body weight to which each of these various species come at the population steady state is not known for all of them, it is clear that a threefold difference in steady state body size does occur between *Hydra littoralis* and *Ch. viridissima* (Slobodkin, 1964).

If animals with flexible body size tend to one particular size, it may be inferred that being either larger or smaller is disadvantageous. That is, it should be possible to define a set of circumstances under which large body size would be favorable for the organisms and a contrasting set of circumstances under which small body size would be favorable. In the case of the hydra, for example, we can suggest that large body size is advantageous if the individual food particles are relatively large so that

too small an animal cannot catch them and hold them. Large body size might also have an advantage if the food supply were extremely sporadic, since during starvation the hydra diminish in body size with time. Large body size is a direct insurance against starvation.

Small body size has a conceivable advantage if the food supply consists of numerous small particles evenly distributed in time. The total feeding surface per unit of living bulk to be maintained is greater in a population of small animals. It is also conceivable that, if the population is threatened in some sense by environmental processes that destroy individual animals regardless of their size, many small animals have an adaptive advantage over a few large ones. Note, I leave adaptive advantage undefined.

All of the species of hydra listed above can be maintained on a ration of brine shrimp nauplii (Artemia sp.). *H. littoralis*, *H. oligactis* and *H. pseudoligactis*, and *H. viridis* can also be maintained on daily rations of young Daphnia. Two species of Daphnia were available, *D. pulex* with newborn animals weighing 3–5 micrograms each and *D. magna* with newborn animals weighing 9–11 micrograms each. The weight of a single Artemia nuplius is around 2 micrograms. By "maintaining the hydra on the food organism" I am here referring to keeping individual animals.

Populations of all of these species can be maintained on Artemia and *D. pulex* and all of them except the green hydra can maintain populations fed on *D. magna*. For the brown hydra, the number of animals maintained on a given number of *D. magna* per daily ration is somewhat larger than can be maintained by the same number of *D. pulex*, but the increase is not quite proportional to the difference in the energetic content of the two species of Daphnia. Within each type of food supply, the size of the population that can be maintained is linearly proportional to the amount of food supplied.

When green hydra populations are fed on *Daphnia magna* there is an initial increase in abundance followed by a decline below the level of a population fed on a corresponding number of *Daphnia pulex*. After approximately fifty days, food consumption stops in the green hydra populations on *D. magna*. The animals are simply too small to catch *D. magna*. If no other food supply is made available, the green hydra population eventually starves to death. If these tiny green animals are put on a diet of Artemia nauplii they can be increased in size to the point that they can again capture and feed on Daphnia. So long as they are consistently well fed on *D. magna*, the green hydra maintain their body size and bud freely. We have, however, never seen a bud from a green hydra feed on *D. magna* young as its first meal.

The inadequacy of *D. magna* as a diet for populations of *Hydra viridis* is, therefore, not a biochemical deficiency of the hydra nor is it related to the biochemistry of the Daphnia except in so far as this relates to the

size of the Daphnia, nor is it due to the inability of the green hydra to grow to a sufficiently large size to eat *D. magna*. It is rather that, in the process of adjusting to a new environment, the green hydra produce very small animals rather than a smaller number of large ones, as if the green hydra are playing the strategy of being numerous and small while the brown ones play the contrasting strategy of being relatively less numerous but larger. It should be noted again that these commitments to specific sizes are not permanently binding in the sense that suitably starved individual brown animals become as small as individual green ones and suitably fed green ones become as large as brown ones.

We have seen a situation in which small size is disastrous for green hydra populations. Clearly we are required to show a correspondingly advantageous situation or be faced with the problem of explaining why green hydra exist at all. When *Hydra littoralis* and *Chlorohydra viridissima* are simultaneously inoculated into a laboratory culture, fed on Artemia nauplii and maintained under a fluorescent lamp, both species increase in number of animals, the brown less rapidly than the green (Slobodkin, 1964; Stiven, 1962). When the number of hydra are approximately twice the number of Artemia per day provided in the food ration, the brown hydra begin to decrease while the green continue to become more numerous. In approximately forty days, the brown hydra have been completely eliminated and the green hydra approach a rather vacillatory steady state.

In any two species system, the available energy must be divided between the two species. By becoming extremely numerous the small green hydra could capture an overwhelmingly large share of the small Artemia nauplii, starving out the brown hydra. This was demonstrated by removing a given fraction of the increase of the two populations daily. This type of removal acts as an increment to the death rate and reduces the number of animals in the residual population. The greater the fraction of the increase removed from the two populations, the longer the persistence of the brown hydra. When 90% of the increase was removed daily, the combined number of hydra of the two species in the population was not too different from what might be found in a control population of only brown hydra, and the two species persisted indefinitely. The competitive outcome between brown and green hydra is not in any obvious way related to any mutual interaction on the biochemical level but is explicable in terms of the relation between number and size of the animals and their food supply.

So far, we have seen that the green hydra have the advantage of greater food-catching surface for their population as a whole. This proves of value when the food supply consists of small particles, but is dangerous if the food supply consists of large particles. The brown hydra have the advantage of individual large size, increasing the likelihood that

each individual animal will acquire some food and decreasing the probability of starving to death between meals.

How can the green hydra counteract the risks of small size? If hydra get smaller and smaller in the absence of food, we might expect that they would eventually get so small as to be unable to eat any metazoan. As a matter of fact, sufficient duration of starvation will give exactly this result. The danger is considerably reduced however by the fact that the green hydra can derive a certain amount of nutrition from the photosynthetic processes of their symbiotic algae.

The significance of the photosynthetic processes of the algae for the green hydra has been demonstrated in three ways. First, we have already indicated that brown hydra are eliminated by green hydra when grown in the same container and fed on Artemia under a fluorescent light. When the experiment is performed under a black cloth wrapper, both species persist (Slobodkin, 1964). Also, when *Hydra viridis* from a stock culture were starved for a twenty-eight day period, the total weight of the hydra was one-third the initial weight for animals kept in cloth wrappers but essentially identical with the initial weight for animals kept in twenty-four hours of fluorescent light. (This experiment is now being repeated at various temperatures and light periodicities and intensities and must be considered cautiously.)

The third demonstration of the role of the algae in green hydra also provides a rough estimate of the relative amount of energy provided by algae. This involved a series of experiments to determine "ecological efficiency." This concept is defined and discussed in fair detail elsewhere (Slobodkin, 1962). Suffice it for the present purpose that ecological efficiency is a fraction with a numerator which is the energy that a predator can get from a population and with a denominator which is the energy consumed by the prey population as food. The evaluation is made at steady state conditions. Whenever this has been evaluated, a maximum value of approximately 10–15% has been found, except in the case of green hydra populations in twenty-four hours of light. If the denominator is taken as the Artemia eaten by the hydra, then the ecological efficiency is approximately 40%. From this I infer that approximately three-fourths of the energy supply of green hydra is derived from the photosynthetic activity of their algal symbionts.

The story of size control in hydra is not yet complete by any means. Preliminary experiments indicate a rather delicate adjustment between the budding mechanisms of the hydra and the rate of photosynthetic activity of the symbionts, for example. We have gone far enough, however, to get the impression that the hydra seem to act as if they are playing some kind of game with their environment, making certain adjustments only when there is reasonable assurance that the risks associated with these adjustments have been covered. This case is simply

an example. Almost any set of ecological phenomena can be analyzed the same way.

In what sense does this manner of speaking contribute insights that would not be available from a more conventional biochemical or physiological approach?

Consider the analogies that are possible between evolutionary processes and changes on the one hand and games on the other. For example, imagine a group of non-chess playing observers at a tournament of mute chess players. It would be possible for such a group of observers to determine the rules of the game fairly readily by simply observing the players. Regardless of superficial differences such as size, shape, and color, there are functional identities between all pawns, all knights, all rooks, etc. There are very limited categories of moves that each of these can make regardless of their relative position on the board or association with each other. Pawns move only forward, polarization of the bishops' activity is at a 45 degree angle to the polarization of the rooks', there is a quasi-helical pattern to the activities of the knights and so on. Occasionally peculiarities might be noted such as "castling," "*en passant* capture," and "queening of pawns" that might be somewhat more difficult to characterize fully.

Once the rules have been determined, it is possible to consider that the game of chess is now "solved" in some sense and go on to the next game or problem. The more complicated the rules of a game, the greater the sense of having solved the game when the rules are determined. To know the rules of a game makes it possible to play the game in the sense that a person manipulating objects on the play area contrary to the rules is not playing the game at all.

All the players of any particular game do play by the same rules. All of them in that sense understand the game. Not all of them win, and this is why games are interesting. One of the standards of quality that can be applied to a game, aside from the intrinsic aesthetics of the manipulative procedures and the social rewards of proficiency, is the subtlety of the relation between playing by the rules and winning. That is, a game in which playing by a set of rules ensures victory is a bore, no matter how complex the rules. The various games involving building a path of one or another color across a field (such as John, Hex, or Bridgit) are dull in this sense. Somewhat better games are those in which the rules or the objects involved embody chance or unknown elements. Card games, dicing games, and contests with a biological element (as races, baseball, golf, etc.) are better games. Betting on their outcome is a worthwhile activity. Some games of this sort are amenable to the construction of interesting game theories in the mathematical sense.

The more delicate board games like chess and Go can be considered either from the standpoint of chance or from the standpoint of strategy.

Such games are played one move at a time. At each player's turn, there is available to him a very large class of bad moves and, if it is an interesting game, a very small class of good moves. In the game of Go, the class of good moves is defined in part by a series of adages or proverbs: "Don't make empty triangles," "Don't peep at the bamboo joint," etc. (Segoe, 1960). To a degree, the same type of adage is used in teaching chess: "In the early game don't move the same piece twice," "Avoid pawns in line" etc. (Mason, 1958). These adages are, in principle, replaceable by a table in which are catalogued all the moves in a very large series of games, the configuration of the board when these moves were made, and information as to the eventual outcome of each game. If the first player makes a particular move, this alters the configuration of the board. This configuration is associated with a certain probability of each player winning or losing. Within the class of moves possible in the new configuration there is a very small subclass which alters the configuration so as to increase maximally the probability of the second player winning. Note that the concept of probability in relation to this type of game does not imply chance events in the game but rather a sampling that has been made of past games. The probabilistic argument as to how to play would, however, work in any case. For example, the poker adage "Never draw to an inside straight," has a statistical justification of a formally similar kind.

Teachers of chess and Go present the adages to their pupils and rationalize them in terms of future events that might occur in the game. "If you do this then when he does that..." They do not make any attempt to equip the student's memory with a probability table for each possible move, although everyone concedes that this is, in principle, possible.

No attempt at all is made to deduce the appropriate moves from the permissible ones. That is, the fact that a knight is permitted to move in a particular way is only in a very trivial way the reason for making a particular knight move at a particular juncture of the game.

The above information is enough to demonstrate that simply saying that evolutionary processes are analogous to a game is meaningless since there are many significantly different kinds of games.

Let us now consider the ways in which the evolutionary process does or does not resemble a game. It is fairly safe to say that all organisms are obeying the same rules in that they have the same or very similar biochemical processes occurring in them. It is possible to define life in terms of these processes in the same way that one may define chess as the placing and moving of pieces on a board in accord with the rules of chess.

In a game, playing by the rules is irrelevant to winning or losing. So, being alive, in the sense of following the biochemical rules that define

life may be irrelevant to "winning or losing" the evolutionary game.

That sentence must be examined very carefully and accepted with all due caution. The biochemical processes occurring in the mammoth, mastodon, or in the saber-toothed tiger were, as far as we can tell, not different in any major way from those occurring in the Indian elephant, African elephant, or pussy cat. One trio is gone, the other is not. It is conceivable that these species became extinct for lack of an enzyme, but it does not seem likely. It has been suggested that simply the accidents of history determine extinction or survival. This position has a misleading plausibility to it. It is exactly the conclusion that a naive observer might come to after deducing the rules of chess and being confronted with the fact that half the players playing at any given moment in time are destined to lose (excluding drawn games).

A rulebook in almost any game is short, definite, and written in declarative sentences. A book on the strategy of the same game is typically much longer, consisting of highly restricted subjunctive statements. Nevertheless, a player who carefully studies a book on strategy becomes much more likely to win than a player who only knows the rules. Is there a definite empirical strategy of evolution? I would like to suggest that the kind of statements I made earlier with reference to the advantages and disadvantages of various body sizes in hydra were in fact elementary examples of evolutionary strategy. The animals that are now alive are successful players at the evolutionary game in that, while we have not yet considered how one can win in evolution, extinction represents a kind of losing.

If we, as articulate observers, can determine what the stakes are in the game, we ought to be able to expand our knowledge of its strategy by examining the properties of the successful players whether or not they are articulate or "intelligent." Note that we are already fairly clear on the idea that the difference between good and bad strategy need not be biochemical in any interesting way.

In the formal mathematical theory of games, the kind of payoff or penalties that the participants in the game receive must be specified. There is a quantifiable reward for winning and some kind of measurable penalty for losing. Lewontin (1961) discussed the evolutionary process from this formal game theoretical standpoint. Can we assign a quantifiable payoff to the evolutionary game?

This question can be answered indirectly. Huizenga (1950), in an historical essay on the role of games in human culture, pointed out the simple, but highly significant fact that games are always played either on a playing field or playing board or some other specified spatial region. Within this region, the only rules that apply are the rules of the game. At the termination of any game, there is a distribution of rewards and

penalties. The players then leave the play area. The value of the reward does not lie in the play area itself but in the non-play world in which the play area is embedded. In other words, for a game to be worth the candle there must be a way of cashing in the winnings and this implies a place must exist other than the gaming board itself.

Animals have no such place. If evolution is a game at all, it is the kind of game that Kafka or Sartre might have created. The only payoff is in the continuation of the game. It is in one sense possible to build a formal game theory, using some suitably defined probability of survival of the population as a whole as the payoff. While this may be logically sound, it runs into considerable operational difficulties.

How would one play a purely existential game in which everything is subordinated to persistence? Neither efficiency, nor complexity, nor power, nor even destruction of one's opponent, can serve as a goal in itself. What is the best strategy in such a game? If we imagine that a player knows only what he himself played at his last turn, his obvious best play is to repeat what he did last time. If the general configuration of the game has changed from one turn to the next, this move may improve his probability of persistence or may reduce it. That is, doing the same thing at each turn, in a game of changing configurations, becomes a kind of random walk. If we consider that the state of a player's fortunes can be specified by an arbitrarily long series of variables and that, at some initial time, each of these variables has a certain value, unless all the players do exactly as they did before, any player that persists in the same move round after round is most probably doomed. Even if all the players stick to their earlier moves, it still is not necessarily the case that any one player will persist in the game. After sufficient time has elapsed, however, each of the surviving players would have locked in on a constant move or a closed cycle of moves. This type of system can persist only if there is no extraneous source of variance being introduced into the configuration of the game. Unless all such extraneous new variance is damped out as rapidly as it is generated by some kind of homeostasis in the system, the configuration of the system will change as a random walk process.

The process of homeostasis, anywhere it occurs, involves adjustments which tend to keep some property of a system constant despite ambient changes. These adjustments, however, involve changes in other properties of the system whose constancy is not of prime significance. (A proper heating system adjusts the rate of fuel consumption so as to keep the temperature of the house constant.) What kinds of things do organisms hold constant, and what kinds of things do they alter for this constancy? Notice we have arrived at an empirical question with a fairly high degree of intrinsic interest. Much of this interest, as in most empirical questions, arises from the context. This context is a somewhat

strange one in its deviations from the usual pattern of scientific argument.

Before we attempt to answer the empirical question it seems advisable to recapitulate the argument which made it matter.

I described portions of an experimental analysis of the regulation of size in several species of hydra. The biochemical and physiological size control mechanisms, to the degree that they are understood, were not really helpful. An essentially teleological approach, asking "What are the advantages and dangers of this or that size?" did seem reasonably fruitful, as if the hydra were making intelligent moves in a game with their environment or, at least, if I were a hydra making intelligent moves in a kind of game with the environment, it would seem reasonable to do what the brown hydra do, given the attributes of brown hydra and to do what green hydra do, given the attributes of green hydra. We then considered the analogy between games and evolution and found that, if the analogy holds at all, it is only with a very restricted kind of game. This restriction of the analogy led us to the statement that the only point of evolution is persistence. This is equivalent to the theatrically unsatisfying end that Darwin came to.

Returning now to the question of what is actually held constant in a constant population by the homeostatic mechanisms of evolution, we are forced by the argument into an apparently paradoxical position. It is undeniable that "fitness" in some sense is either held constant or maximized in the evolutionary process. Unfortunately, the word "fitness" has been used to mean everything from physiological vigor to reproductive capacity (Birch, *et al.*, 1963). If we accepted any of these as the thing which the homeostatic mechanisms of organisms are "trying" to hold constant, we would also be saying that this was the payoff for the evolutionary game. We can get around this difficulty if we consider that homeostatic ability itself is the thing which evolutionary homeostasis is "trying" to hold constant. The apparent paradoxes generated by all other definitions of "fitness" now disappear. Even the concept of "reproductive value" increase (Fisher, 1930) takes on the character of a special case. There is the danger that, in eliminating the paradoxes, we have eliminated all content and questions. This is not the case, although the form of evolutionary questions must, of necessity, now alter, as will be discussed later.

This conclusion is not new. Bateson (1963) points out that there seems to be a hierarchy of adjustment mechanisms all acting to preserve flexibility to environmental changes. The time required for each of these various adjustment mechanisms to respond to an alteration in the environment is inversely related to the range of environmental changes in which the mechanism in question will respond. All of the responses are in such a direction as to maximize the ability of the evolutionary unit (as

defined in Thoday, 1953) to respond to subsequent environmental change. On the shortest time scale and narrowest environmental range are the rapid physiological changes comparable to those that occur over a diurnal cycle in man. Individual organisms may become acclimatized to a shift in environmental circumstances. In addition, certain kinds of alterations in environment result in numerical alterations in the number of organisms in the population which bring about changes in the state of the organisms themselves.

Should an environmental change be an essentially permanent one, the evolutionary unit will adjust by gene frequency changes to restore the flexibility of the acclimatization mechanisms just as they restored the flexibility of the short range physiological adjustment mechanisms. Presumably, any genetic change will reduce the capacity for further genotypic change. Bateson speculates that flexibility of the genotype would itself be selected for. This would also be anticipated from our argument as to the significance of homeostatic ability *per se*.

The effect of adjusting each of the higher levels in this hierarchy of feedback devices is to restore the flexibility of response to the next lower level. Bateson says "...there must be an *economics of somatic flexibility* and this economics must, in the long run, be coercive on the evolutionary process. . . . The organism or species would benefit (in survival terms) by genotypic change that would *simulate* Lamarckian inheritance . . . . Such a change would confer a bonus of somatic flexibility and would therefore have marked survival value." (Italics his.) This is, in effect, identical with the conclusion derived from consideration of the existential game.

Bateson is not the only author to derive approximately this conclusion. Thoday (1953) states that survival probability is the only appropriate measure of fitness. Waddington (1957) says "...we are in fact suggesting that all natural selection is in fact a selection for the ability of the organism to adapt itself . . . . in the environment in which it finds itself." Lewontin (1957) indicates clearly that he believes the central problem of evolution to be the development of a theory of homeostasis which maintains constant the survival potential of the evolutionary unit.

With the possible exception of Thoday and Bateson, authors writing about this problem seem to back away from the conclusion that the homeostatic devices of the evolutionary unit are themselves being maintained by homeostatic mechanisms. They tend to substitute more easily measured physiological properties as the thing whose constancy is of prime evolutionary significance. Waddington (1957) condemns Thoday for insisting on an almost unmeasurable concept as an evolutionary goal and feels that this is a misuse of the term homeostasis.

Waddington's concept of developmental homeostasis is somewhat different from the notion of flexibility discussed here (Waddington, 1957). He is referring to the homeostatic maintenance of constancy in development so that a fairly broad spectrum of environmental alterations will be compensated for by the organism during the developmental process. From our present standpoint, this is most significant as implying that the terminal resultant morphology of this process is the morphology of maximal homeostatic capacity (i.e., flexibility) for the environment in question. That is, if developmental homeostasis in the sense of Waddington is highly developed, it implies that the terminal morphology is of major significance in fitness while, if it is not, there is the implication that other methods are available for coping with environmental variability. This is of interest in that it indicates the existence of a class of meaningful ecological questions that can be formulated and in part answered from knowledge of the organisms themselves, having reference to the organisms' environment without involving direct environmental measurement.

The contention that all of the adjustment mechanisms of organisms and populations function to maintain the flexibility of the evolutionary unit may only be valid for biological systems although Ashby's homeostat operates in the same general way, but without the hierarchical property (Ashby, 1952).

No insurmountable logical problem is generated by holding homeostatic ability constant nor are normal assumptions of the physical-chemical bases of life violated.

It might be argued that all biological statements other than those explicitly stated in terms of chemical reactions are of necessity invalid. This position would deny that strategy matters in chess. It is a kind of fundamentalist mechanism and represents an almost complete circle in the history of biology.

In the late nineteenth century conflict between mechanism and vitalism, one of the strongest general arguments against vitalists was that their insistence on the intrinsic impossibility of biological analysis closed a series of intellectual pathways while a mechanist or "agnostic" position permitted these pathways to be explored.

To close the door to enquiry in the name of mechanism would seem a tragedy.

Lerner's concept of genetic homeostasis (Lerner, 1954; Wallace, 1963) is analogous to Waddington's developmental homeostasis in that both are concerned with the mechanisms by which those components of fitness which are appropriate for long term average environmental conditions avoid altering too rapidly to accommodate to short term fluctuations. In the absence of mechanisms of this type, changes in the environment of a species can only be met by direct changes in gene

frequencies. As a matter of fact, for some morphological features this seems to be the case. Human eye color or blood type, however complex the actual genetic or biochemical mechanism of their determination, are fixed within an extremely narrow range for the duration of the individual's life and cannot be altered without selection. Others, for example the human waistline circumference, while related to genotype are notoriously flexible during the life of an individual.

The expressivity of the genes controlling different morphological properties is very different. That this is not a property of specific kinds of genes can be demonstrated by the differences that exist between species in precisely which properties are variable during the life of an animal and which are not. In the context of maintenance of homeostatic ability of the evolutionary unit, a morphological property controlled by genes of low expressivity would be assumed to have its primary significance with reference to an environmental variable which is likely to fluctuate at intervals that are relatively short compared to the generation time of the organisms in question.

Levins (1961) has discussed this problem in an interesting way. He says

> ". . . Natural selection in a slowly changing environment in general permits a species to follow the changes and improve its fitness. But if the environment changes too rapidly, a response to selection may be disastrous. Suppose that a population of butterflies has one summer and one winter generation per year. If there are genetic differences in winter and summer viability, the winter generation will be modified by selection to produce a summer generation even more adapted than their parents to winter conditions, but less well adapted to the summer environment in which they live. We have designated this the Epaminondas effect after the small boy who always did the right thing for the previous situation. Here the optimal situation would be one in which the same genotype produces either winter or summer forms depending on say temperature of early development."

In summary, from a consideration of the relation between physiological, ecological, and genetic mechanisms by which populations adjust to their environments it seems likely that only flexibility or homeostatic ability is always maintained at a relatively high level by the evolutionary process. All other features of all organisms are expendable to this end. The response of an evolutionary unit to any environmental change seems to involve the simultaneous initiation of a series of physiological, ecological, and genetic changes all acting to restore the ability of the evolutionary unit to respond to subsequent environmental changes. These various flexibility-restoring mechanisms occur at different rates so that certain of them are essentially not influenced by short term fluctuations in the environment.

It also seems likely that a major mechanism of flexibility maintenance is that the more slowly reacting homeostatic devices tend to restore the flexibility of response of the more rapidly responding ones. This seems to

occur by a readjustment of the norm of the more rapidly acting mechanism, as if one had a slow response thermostat that would make seasonal adjustments in the setting of a rapidly responding thermostat. The more slowly responding mechanisms are in some sense "deeper" properties of the organisms, involving more widespread adjustments of various properties of the individuals and the population (*cf.* Bateson, 1963).

Gene frequency changes in this scheme would appear as last resort responses to essentially permanent environmental changes. The process of speciation is a special case of gene frequency changes in allopatric populations.

The adjustment mechanisms and their hierarchical interactions can be demonstrated in part for many organisms, and in some cases it can be shown that flexibility is in fact restored by the adjustment processes. Nevertheless, stating the case as strongly as I have done here involves frank speculation.

While it may be speculative it is not merely a semantic exercise. This can be shown in two ways.

First, empirical predictions are generated. For example, it would be predicted from this scheme that short-lived organisms would tend to have genetic systems involving higher expressivity than related long-lived organisms. It would also be predicted that speciation and extinction of populations would always be accompanied by relatively rapid and permanent environmental changes.

Second, alternative speculations involving the goals of the evolutionary process are denied. If the mechanism I have outlined is valid, then the absence of morally and philosophically satisfying conclusions about the ultimate meaning of evolutionary history are not weaknesses of biological theory nor are they due to a shortage of biological facts. They are simply wrong. Evolution, in fact, is simply a consequence of the general homeostatic ability of organisms combined with the biochemical properties of genetic material.

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AMERICAN SCIENTIST, 52, 1964

POLYMERIC SULFUR AND OTHER POLYSULFIDE POLYMERS

By ARTHUR V. TOBOLSKY

When rhombic sulfur is heated in a sealed evacuated tube it first melts to a pale yellow liquid at 113 °C. This liquid consists primarily of S_8 rings. It is a liquid of normal low viscosity, and the viscosity decreases slightly with increasing temperature up to about 159 °C. At this point, there is a fairly abrupt and very large increase in viscosity, followed by a gradual decrease at still higher temperatures, as shown in Figure 1. The viscosity changes are completely reversible [1]. The temperature of 159 °C. is regarded as a transition temperature or floor temperature for reasons which will soon be clarified.

Equilibrium Polymerization of Sulfur

This phenomenon has for long been regarded [2] as due to an equilibrium between sulfur in ring form, S_8, and sulfur in a long chain polymeric form, —S S S S S S S S S S—. Gee [3] considered the equilibria between rings and chains utilizing two separate thermodynamic treatments, one valid *below* the transition temperature and the other valid *above* the transition temperature.

Tobolsky and Eisenberg [4] showed that the final results obtained by Gee could be developed from a single and simple thermodynamic theory valid both below and above the transition temperature. The equilibria considered were

$$S_8\,(\text{ring}) \overset{K}{\leftrightarrows} \cdot\, \text{S S S S S S S S} \,\cdot\; S_8(\text{diradical})$$

$$S_8\,(\text{diradical}) + S_8\,(\text{ring}) \overset{K_3}{\leftrightarrows} S_{16}\,(\text{diradical})$$

$$S_{16}\,(\text{diradical}) + S_8\,(\text{ring}) \overset{K_3}{\leftrightarrows} S_{24}(\text{diradical})$$

$$\cdots\cdots\cdots$$

$$S_{8n}\,(\text{diradical}) + S_8\,(\text{ring}) \overset{K_3}{\leftrightarrows} S_{8(n+1)}(\text{diradical})$$

$$\cdots\cdots\cdots$$

One notes that two different equilibrium constants are introduced, the constant K for initiation (opening of the ring) and the constant K_3 for propagation (addition of ring to diradical of any size).

By simple application of the law of mass action (using moles per kilogram as the concentration unit) the following two equilibrium equations were derived which are valid at all temperatures:

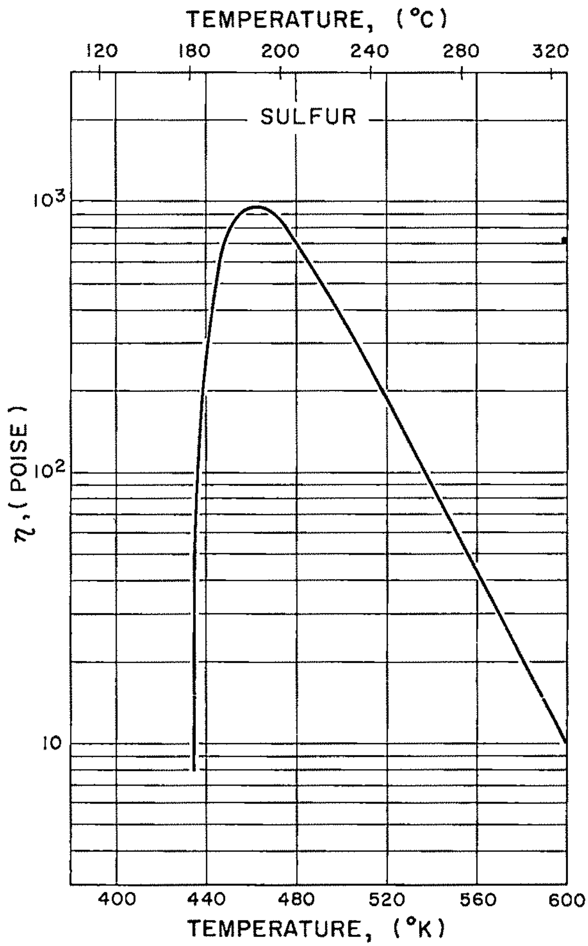

FIG. 1. Viscosity versus temperature for sulfur in the region of transition.

$$P = 1/(1 - K_3M) \tag{1}$$

$$M_0 = M + KM/(1 - K_3M)^2 \tag{2}$$

In equations (1) and (2) the following symbolism is used: P is the number average degree of polymerization (in S_8 units) of the polymeric diradicals, M is the equilibrium monomer (S_8 ring) concentration and M_0 is the total concentration of S_8 units in both monomer and polymer. The quantity M_0 is therefore a constant equal to 3.90 moles per kilogram.

The quantities K and K_3 are equilibrium constants which are functions of temperature only. They are given by the following van't Hoff equations:

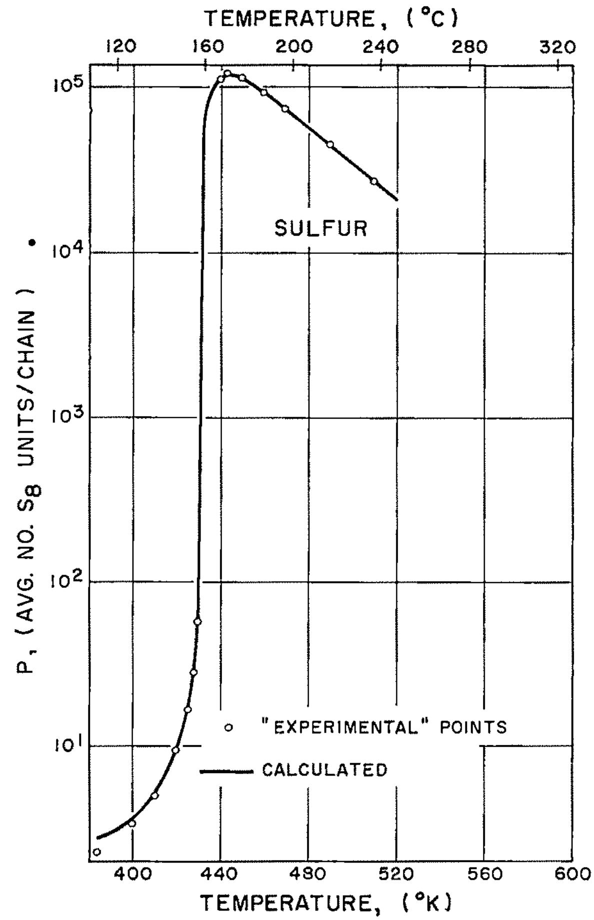

FIG. 2. Degree of polymerization versus temperature for sulfur in the region of transition.

$$\ln K = -\Delta H/RT + \Delta S/R \tag{3}$$

$$\ln K_3 = -\Delta H_3/RT + \Delta S/R$$

$$\Delta H = 32{,}800 \text{ cal/mole} \qquad \Delta S = 23 \text{ cal/deg. mole}$$

$$\Delta H_3 = 3170 \text{ cal/mole} \qquad \Delta S_3 = 4.63 \text{ cal/deg. mole}$$

It is quite remarkable that equations (1), (2), and (3) give a complete description of Gee's *P versus T* and *M versus T* curves, *including* the appearance of a sharp transition at 159°C. The results are shown in Figures 2 and 3.

The ring chain equilibria in selenium have been analyzed in a similar manner [5]. Copolymerization of sulfur and selenium has been successfully explained [5a].

Quick-Quenched Sulfur

When molten sulfur is heated above 159°C., preferably to 200°–250°C., and then rapidly quenched to a temperature of about −20°C., a transparent elastic substance is obtained. This easily performed experiment is a favorite lecture demonstration in freshman chemistry courses.

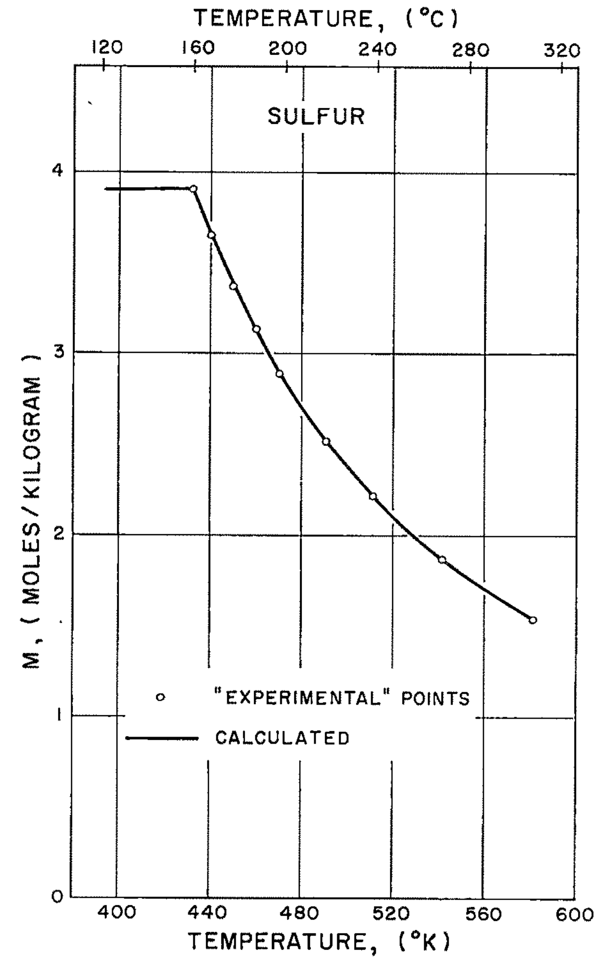

FIG. 3. Equilibrium monomer concentration versus temperature for sulfur in the region of transition.

Unfortunately "elastic sulfur" hardens when kept at room temperature, with formation of visible crystals of rhombic sulfur. If this substance is now extracted by CS_2 a portion of the sulfur dissolves as S_8 rings. The remainder of the sulfur is insoluble in CS_2 and for many years has been recognized as polymeric sulfur.

Until very recently, the hardening of elastic sulfur at room temperature with the formation of visible crystals of rhombic sulfur has been regarded as a reversion of polymeric sulfur to rhombic sulfur. Although reversion of polymer to ring occurs rapidly at higher temperatures such as 90°C., the reversion at room temperature is quite slow. The rapid hardening at room temperature arises from a different mechanism. To explain this one must consider more carefully the composition and structure of quick-quenched sulfur.

In the temperature range of 200–250°C., at which the sulfur is heated before quenching, it consists of a liquid mixture of S_8 rings and long chains. A quick chilling of the sulfur accomplishes two results: it "freezes" the equilibrium between ring and chain, characteristic of the high temperature mixture; we have shown that it also freezes the amorphous or liquid-like physical state of this mixture [6]. This mixture has a glass transition temperature of about −30°C. If kept below this temperature, the chemical and the physical state of this mixture will remain stable for indefinitely long periods of time.

Below the glass transition temperature of amorphous materials these substances are in a "frozen" though not crystalline condition. The motions of molecules or molecular segments are restricted to short range vibrations, and changes in the chemical or physical state of the substance occur extremely slowly. Amorphous substances are hard and glassy (high modulus of elasticity) below their glass transition temperatures. Amorphous substances of low molecular weight are liquid above their glass transition temperature; amorphous substances of high molecular weight (polymers) are rubbery above their glass transition temperature.

Above −30°C., the quick-quenched amorphous mixture of sulfur is in a rubbery state because the S_8 rings which are in a metastable liquid state are acting as a plasticizer for the polymer and thereby lowering the glass transition temperature to −30°C. [6].

The pure polymer, which can be obtained by extracting the quick quenched sulfur with CS_2, has a glass transition temperature of about 75°C. [6]. The polymer is actually sold under the trade name of Crystex. This substance is by no means rubbery at room temperature.

The hardening of elastic sulfur above its glass transition temperature occurs, not because of a reversion of polymer to rhombic sulfur, but because the dissolved S_8 in its metastable liquid condition is not soluble in the polymeric sulfur. It crystallizes out as rhombic sulfur. The mixture of polymeric sulfur interspersed with small crystals of rhombic sulfur is not rubbery. The polymeric sulfur may also change from an amorphous to a semicrystalline condition.

Polymeric sulfur *does* revert to rhombic sulfur fairly rapidly above 90°C. The specific rate constant for the cleavage of the polysulfide linkage has been obtained from studies of organic polysulfide polymers,

Polysulfide Polymers

Polysulfide polymers can be obtained by reacting a suspension of an organic dihalide such as $ClCH_2CH_2Cl$ in aqueous sodium polysulfide [7]. The suspension is facilitated by use of a small amount of weak wetting agent.

We have prepared crosslinked rubber networks by reacting ethylene dichloride, $Cl\,CH_2 \cdot CH_2Cl$, and small amounts of 1,2,3 trichloropropane, $Cl\,CH_2 \cdot CHCl \cdot CH_2Cl$, with sodium tetrasulfide. The structures produced are networks of the following kind:

```
   —CH2CH2SSSS CH2CH2SSSS CH2CH2SSSS CH2CH CH2SSSS CH2CH2SSSS—
                                         S
                                         S
                                         S
                                         S
—CH2CH2SSSS CH2CH CH2SSSS CH2CH2SSSS CH2CH CH2SSSS CH2CH2SSSS—
                 S
                 S
                 S
                 S
```

The polymer is produced in the form of coagulated crumb. Considering that it is a crosslinked infinite network it is surprising that this crumb can be readily compression-molded into rubber sheets at 130°C. This is possible because the tetrasulfide linkages are labile; they break and reform very readily at this temperature.

We have determined the specific rate constant for the cleavage of the tetrasulfide linkage in a quantitative manner by the use of the technique of stress decay at constant extension [8]. The rubber is stretched to a fixed length and the stress required to maintain that length is measured as a function of time at various temperatures. The decay of stress is a reflection of the breaking and reforming of the tetrasulfide linkage. By analysis of the stress relaxation data we have found that the specific rate constant for the homolytic cleavage of the tetrasulfide linkage is [8]:

$$k' = 5.86 \times 10^{12} \exp\left(-25.9 \text{ kcals}/RT\right) \text{ sec}^{-1} \tag{4}$$

By comparing polymers prepared from sodium disulfide rather than sodium tetrasulfide we have determined that the disulfide linkage is much more stable than the tetrasulfide or trisulfide linkage [9, 10]. A possible explanation for this result is that the cleavage of a trisulfide or tetrasulfide linkage may be stabilized by the formation of a three electron sulfur–sulfur bond, whereas this is not possible for a disulfide linkage.

$$\text{—SSSS—} \rightleftharpoons \text{—SS}\cdot + \cdot\text{SS—} \rightleftharpoons \text{—S}\cdots\text{S}+\text{S}\cdots\text{S—}$$

Studies of the viscoelastic properties of polymeric sulfur slightly crosslinked with arsenic [11] or with phosphorus [12] also indicate that inter-

change reactions are occurring amongst the polysulfide linkages of these networks.

Sulfur as a Plasticizer for Polysulfide Polymers

We were able to verify our postulate that liquid sulfur can exist in a metastable supercooled state in the presence of polymers other than polymeric sulfur [13]. We prepared the crosslinked coagulated crumb from ethylene dichloride, 1,2,3 trichloropropane and sodium tetrasulfide. Sixty parts by weight of this was then admixed with 40 parts of elemental rhombic sulfur. The mixture was compression molded at 130°C. to give transparent rubbery sheets. This composition was indefinitely stable. No rhombic sulfur was observed to crystallize out on the surface after 18 months of storage at room temperature. Some rhombic sulfur *did* crystallize out of the 50-50 mixture after two months.

The sulfur in this sheet was present in elementary S_8 ring form; it did not enter the polysulfide chain. It was completely extractable with CS_2 at room temperature.

The elemental sulfur was present in the sheets in a liquid condition. This was verified by measuring the specific volume of the unplasticized polymer and the plasticized polymer. Using a simple additivity law, one could determine the specific volume of the dissolved elemental sulfur. This agreed very well with the specific volume of ring sulfur in its liquid state obtained by extrapolating the specific volume data for liquid sulfur to room temperature.

Summary

Elemental sulfur exists in many chemical forms (rings, chains) and also in a variety of physical states. The supercooled liquid state of the ring structure, S_8, is of especial interest. The lability of the polysulfide linkage is very great. This accounts for the rapid establishment of ring-chain equilibria and also for the reactivity of sulfur with other substances.

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AMERICAN SCIENTIST, *52*, 1964

DNA AND THE CHEMISTRY OF INHERITANCE

By BARRY COMMONER

LIVING organisms are unique, among the known forms of matter, because they are capable of creating their own specific, highly organized structure out of substances taken from far more disorganized surroundings, and can transmit this capability to their offspring. Perhaps the oldest and most profound theoretical problem in biology is the effort to explain the curious paradox that, despite its unique capability for self-duplication and inheritance, a living organism is nevertheless a mixture of substances which are separately no more possessed of these properties than are the more prosaic molecules that never occur in living cells. This question has been at the root of a long train of experiments, debates, and speculations that begins in classical times and continues unbroken through the development of present-day "molecular biology."

The basic issues are simply stated. If the component parts of a cell are not themselves alive, whence come the life-properties exhibited by the whole? Apart from the untenable notion of a mystic non-material "vital force" which supposedly animates the otherwise dead substance of the cell, the debate has elicited two main positions: (*a*) There is, in fact, some special cellular component which possesses the fundamental attribute of self-duplication, and which is therefore a "living molecule" and the basic source of the life-properties of the cell. (*b*) The unique properties of life are inherently connected with the very considerable complexity of living substance and arise from interactions among its separable constituents which are not exhibited unless these components occur together in the complex whole. In this view, only the entire living cell is capable of self-duplication.

There is at this time a widespread impression that this issue has now been resolved and that the cell does indeed contain a component—DNA—which, according to the theory of the "DNA code," possesses the basic attribute of life, self-duplication, and which guides the activities of all the inheritable processes of the cell.

The importance of this conclusion is self-evident, for it would answer, at last, the basic question of the origin of the unique properties of life, and, if correct, should lead to unprecedented technological control over these properties. It is appropriate, therefore, to ask what criteria are required to establish that a molecule, such as DNA, is capable of self-duplication, to examine the degree to which the available evidence meets such criteria, and to determine whether the undoubted importance

of DNA in the biology of inheritance may be due to some properties other than those attributed to it by current theory.

The present paper is a summary of some considerations of these questions. In it, I shall develop evidence which indicates that, despite the widespread conviction to the contrary, the theory of the "DNA code" does not explain the biological property of self-duplication or provide a description of the role of DNA in the cell and its relation to inheritance which adequately accounts for all the relevant data. These considerations also lead to certain new proposals which help to remedy these defects, and which suggest new experiments that may contribute to a further elucidation of the processes of self-duplication, development, and inheritance.

The Problem

The basic problem to be examined is the molecular origin of an apparently unique property of living organisms—self-duplication. Since self-duplication is a concept of biological origin, it is appropriate that criteria for determining that such a process exists be derived from biological evidence.

A typical biological self-duplicating system consists of a specific cell, and the environmental constituents (for example, water and nutrients) that are essential if growth and replication are to occur. According to the concept of self-duplication, only one of the several constituents of such a system—the cell—is the source of the inheritable specificity that appears in the daughter cells which are the product of the replicative event. The parent cell is regarded as being capable of *self*-duplication because it is the *sole* source of the transmissible specificity of the daughter cells. Other constituents of the system, such as water and nutrients, are also essential to the replicative event, but, unlike the parent cell, they do not appear to contribute transmissible specificity to the products of that event. Thus, in determining whether a particular agent is capable of self-duplication, the chief issue is the source of the inheritable biological specificity which is transmitted to the progeny of the replicative event in which it participates.

Recent progress in the analysis of biochemical changes associated with the inheritance of biological characteristics establishes useful connections between biological specificity and the properties of biochemical systems. For example, a number of specific biological characteristics, such as the ability to grow in a particular medium, have been shown to result from the presence or absence in the organism of an enzymatic protein of specified structure. There is also evidence that the specific structures of other macromolecules, such as nucleic acids and polysaccharides are inherited, as are patterns of biochemical reactivity, or metabolic pathways.

In general, then, at least some of the inheritable characteristics of living organisms are known to arise from the *biochemical* specificity of the cell. Such biochemical specificity occurs in two major forms: molecular structures and molecular reactions. Structural, or *static*, biochemical specificity is represented by the particular order of the constituent residues that comprise linear polymers such as nucleic acids and proteins; other aspects of these molecules' structures, such as their secondary or tertiary configurations, are usually directly dependent on the primary structure, or order of residues. In such molecules, a very large number of primary structures (i.e., different sequences of the residues comprising the polymer) have equal thermodynamic probability, so that the static specificity of a given molecule represents the choice of a particular sequence among very many possible ones.

Cells also possess *kinetic* biochemical specificity which is represented by the preferential occurrence of particular reactions of the immense number that are thermodynamically possible among the cell's numerous reactive constituents. Such kinetic biochemical specificity is represented by a particular metabolic pathway such as the Krebs cycle or the series of reactions which lead to the synthesis of a particular amino acid from carbon and nitrogen sources. Defined in this way, biochemical specificity is equivalent to the term "information content" as employed in recent discussions of molecular aspects of genetics.

The establishment of either static or kinetic biochemical specificity requires the regulation of the rates of the relevant intracellular reactions. Such regulation determines which ones of the possible alternative reactions actually occur at appreciable rates in the cell, and thereby govern the outcome of cellular chemical events. It will be useful, in what follows, to refer to a cellular agent that is capable of such regulation as a *regulatory* component, its participation in the chemistry of the cell being indicated by the symbol (═▷). If a regulatory component or system is also capable of determining the specificity *of its own synthesis*, then the regulatory agent is itself replicated, and therefore may be transmissible in inheritance. An agent with these properties will be termed a *germinal* component or system, and its participation indicated by the symbol (≡▷). The participation of a component, or system, which is essential to a biochemical process, but which nevertheless does not contribute any specificity to the product will be indicated by the symbol (—▷). These relationships, which are summarized in Figure 1, are useful in an analysis of the origins of static and kinetic biochemical specificity.

The general problem to be solved in a molecular analysis of self-duplication and inheritance is the identification of the germinal components, or systems, which govern the biochemical specificity of the cell. In order to avoid *a priori* assumptions regarding the properties of this class of components, it is useful to consider first the properties of

the more general class of regulatory components, and then to determine which members of the class conform to the additional requirements of a germinal component, or system.

Regulatory Components

The extensive literature of biochemistry, in particular recent analyses of metabolic control [1], provides fairly detailed evidence about the mechanisms which regulate kinetic biochemical specificity. A simple

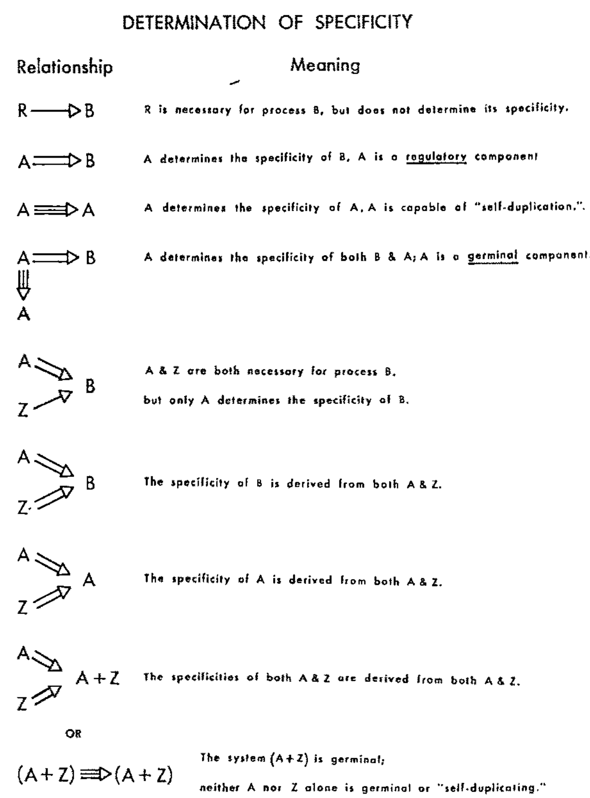

FIG. 1. Diagram to summarize definitions of symbols for nonregulatory participants, regulatory and germinal components of systems involving transfer of biochemical specificity.

and well-known example is summarized in Figure 2. In organisms such as yeast and lactobacillus, which are genetically restricted to the glycolytic production of alcohol and lactic acid respectively, this kinetic specificity is achieved by the activity of specific enzymes of the cell. Both paths involve the same initial reactants (pyruvic acid and DPNH). In yeast, which has a considerable concentration of the enzymes pyruvic decar-

boxylase and alcohol dehydrogenase, the reaction leads exclusively to alcohol production. In lactobacillus, another enzyme, pyruvic dehydrogenase, predominates so that lactic acid is the main product of glycolysis. This example symbolizes the well-established generalization that enzymes, which greatly accelerate specific reactions, are powerful regulators of kinetic biochemical specificity. It illustrates how the static specificity of the enzyme protein (on which its catalytic activity depends) is transmitted to the kinetic specificity of the cell.

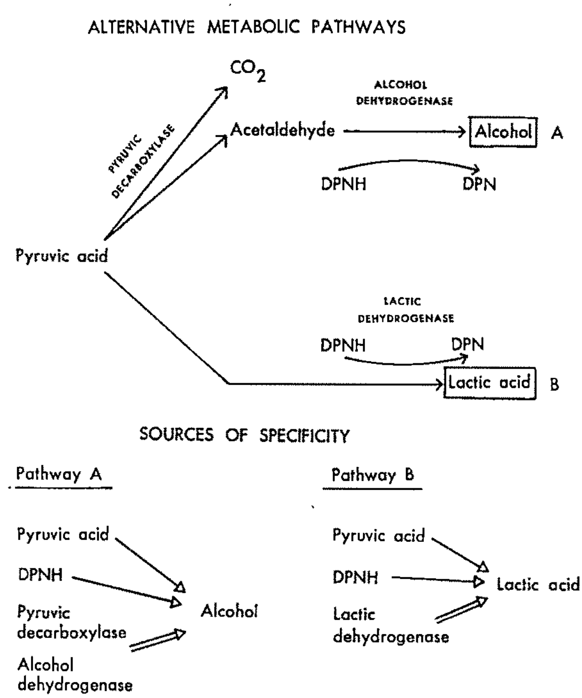

FIG. 2. Diagram to illustrate a simple example of the regulatory effect of enzyme specificity on the determination of kinetic specificity in a metabolic system. Symbols in lower diagram are in accordance with definitions summarized in Fig. 1.

When both of the foregoing sets of enzymes are active in a cell, regulatory effects must be localized in some other component of the system. Krebs [2] has shown that when an agent, such as DPN/DPNH, which serves as an essential coenzyme in the activity of both alcohol and lactic dehydrogenase, is shared among several metabolic processes, individual processes may predominate at a particular concentration of the agent. Thus, in the case illustrated in Figure 2, at sufficiently low concentrations of DPNH, we may expect the reduction of pyruvic acid to lactic acid to

proceed to the exclusion of the alternative pathway, for, although both lactic and alcohol dehydrogenase are present, the dissociation constant with respect to DPNH of the former is smaller by an order of magnitude. Hence, under these conditions, the coenzyme reacts preferentially with one of the two available enzymes, and one of the two possible metabolic pathways is enhanced.

An extensive family of similar regulatory effects is known, especially with reference to carbohydrate catabolism [3]. A summary of these effects leads to certain useful generalizations: (*a*) While enzymes are powerful regulatory components, those which catalyse basic processes in carbohydrate catabolism, e.g., dehydrogenases, are often present in excess and are therefore incapable of regulating changes in the course of metabolism which accompany specific physiological events. (*b*) Catalytically active intermediates, especially those which are involved in several metabolic processes are often present in low concentrations, which, by limiting the rate of certain possible alternative reactions, determine the predominant metabolic pathway. (*c*) Nucleotide coenzymes and phosphate carriers, which are often decisive branch points in metabolism, are particularly important determinants of metabolic pathways. (*d*) Important regulatory effects are also exerted by the balance between alternative chemical states of catalytic intermediates (e.g., DPN/DPNH, TPN/TPNH, and ADP/ATP balances), which are in turn closely associated with the over-all rates of metabolic degradation of carbohydrate starting materials. (*e*) The synthesis or activity of enzymes involved in the formation of certain metabolic end products may be regulated by feedback systems governed by the concentrations of these end products.

There is a growing literature concerning the origin of the static biochemical specificity embodied in the nucleotide sequence of nucleic acids and in the amino acid sequence of proteins. Since these problems are, of course, central to a subsequent consideration of the nature of germinal components, discussion of them will be deferred, except to note here that the relevant observations introduce a new type of regulatory component, the nucleic acids primers, or "templates," which participate in the determination of the biochemical specificity of these polymers.

Thus, the following general types of components and systems may serve as *regulators*, i.e., as determinants of kinetic or static biochemical specificity: (*a*) enzymes, (*b*) nucleic acid "templates," (*c*) the concentrations of catalytic metabolic intermediates, especially nucleotides, (*d*) the concentrations of biosynthetic end products. Which of these classes of regulators also determine the specificity of their own synthesis and are therefore capable of serving as *germinal* agents?

Since the present paper is restricted to a consideration of the germinal role of DNA, further discussion will be confined to those regulatory

TRANSFER OF DNA SPECIFICITY

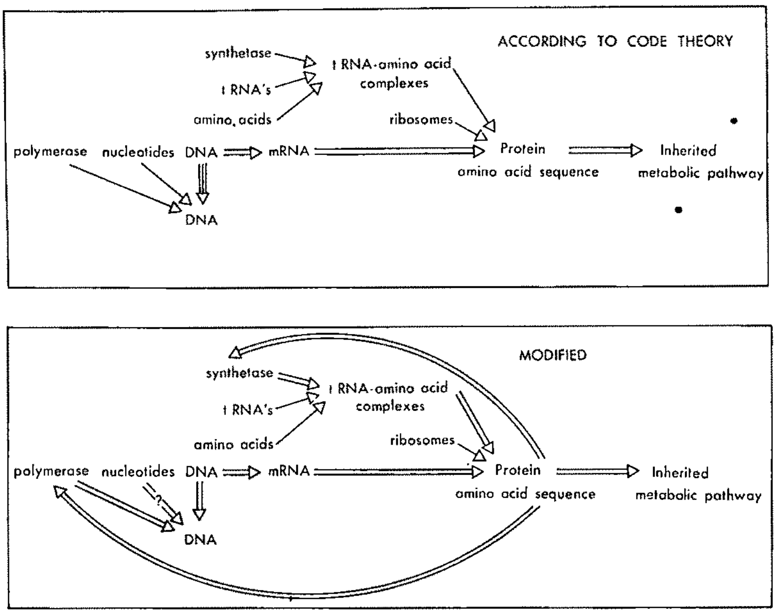

FIG. 3. Upper diagram illustrates the transfer of biochemical specificity in the system proposed by the theory of the DNA code, which holds that the biochemical specificity of DNA (its nucleotide sequence) is the sole source of specificity in the replication of DNA and in the synthesis of protein, the specificity of the latter determining a particular inherited metabolic pathway. In this scheme, DNA is itself a germinal self-duplicating component and the sole source of the cell's inherited biochemical pattern.

B) Lower diagram illustrates modification of the system suggested by the evidence discussed in the text. This scheme illustrates the consequences of evidence that part of the biochemical specificity of DNA originates in the polymerase enzyme which catalyzes DNA synthesis, and possibly in the relative availability of the several requisite nucleotides. Also shown is the effect of contribution of specificity originating in amino-acyl RNA synthetase, an enzyme essential to protein synthesis. In this scheme DNA is no longer the sole source of biochemical specificity for either DNA or protein synthesis; it is neither a germinal, self-duplicating component, nor the exclusive determinant of the cell's biochemical specificity. Instead, these effects have a multi-molecular origin and the entire system requires pre-existing specificity in both DNA and at least two protein enzymes. Symbols according to definitions given in Figure 1.

processes which may mediate the participation of DNA in inheritance. This limitation should, however, be recognized as an arbitrary expedient; it is not intended to imply that only DNA can serve as a germinal component. Indeed, as will become apparent later, we shall be required seriously to modify this simple concept.

The Theory of the DNA Code

There is a well-known theory that DNA governs biological inheritance because the static specificity of its molecular structure—the sequence of nucleotides in a particular segment of DNA—is the sole source of both the static specificity of newly synthesized DNA and of the kinetic specificity of particular metabolic reactions in the cell. The scheme proposed by the theory of the DNA code is summarized in Figure 3A, using the symbols defined earlier. The theory leads to the notion that DNA is a "self-duplicating molecule" because it holds that DNA is the only source of the specificity of newly synthesized DNA. Moreover, according to this scheme, the static biochemical specificity of a given protein, which is due to its amino acid sequence, arises ultimately from a single source—the nucleotide sequence of a particular DNA segment. These conclusions are, however, valid only if it can be shown that no other *regulatory* components participate in DNA synthesis or in the processes which intervene between the DNA "template" and the synthesis of a particular protein. It becomes important, therefore, that the experimental evidence be scrutinized with this question in mind [4].

Three classes of components are operative in *in vitro* DNA synthesis: The DNA primer or "template," the DNA polymerase enzyme and the necessary deoxyribonucleotides. That the biochemical specificity of the newly formed DNA (i.e., its nucleotide sequence) is under the partial control of the primer is evident from a number of Kornberg's observations of similarities in the composition of primer and product DNA [5]. However, the correspondence is not always complete, and in any case no method yields more than very scattered data regarding nucleotide sequence. Moreover, it has not been possible conclusively to demonstrate the actual replication of a biologically active DNA in this system. Thus, evidence from *in vitro* synthesis of DNA shows that the primer DNA is a regulatory component in this process and influences the biochemical specificity of the DNA product. But there is no direct evidence that the primer exerts the total control over the specificity of the product, which is required of a *germinal* component [6].

The conclusion that DNA is a self-duplicating germinal component also calls for the negative demonstration that the DNA polymerase enzyme, which is an obligate participant in DNA synthesis does *not* contribute to the biochemical specificity of the product (i.e., its nucleotide sequence). However, certain experiments with the DNA polymerase system, reported by Kornberg and his collaborators, lead to the empirical conclusion that the enzyme may also contribute specificity to the DNA product.

When the enzyme, DNA polymerase, is added to a mixture of deoxyadenylic and deoxythymidilic acids, in the absence of a DNA primer,

DNA synthesis is observed after a lag period [7]. The product is an A-T copolymer with a regular alternating sequence of nucleotides: ... ATATAT... When a corresponding experiment is performed with a mixture of deoxyguanylic and deoxycytidilic acids, two polynucleotide products are formed: ...GGGG...and...CCCC.... In these cases, each of the DNA products has a regular and quite specific, albeit simple, nucleotide sequence and therefore possesses biochemical specificity. Since DNA primer is absent, this specificity must originate in the enzyme, which, in the absence of a primer, appears to catalyze specifically certain internucleotide bonds.

These results indicate that, in a system which synthesizes DNA in the presence of primer and enzyme, both of these constituents may be expected to contribute to the specificity of the DNA product. Direct evidence of the dual origin of DNA specificity in such a system requires observation of the effects of experimental variation of the specificity of the two components on the nucleotide sequence of the DNA product. While the absence of the necessary techniques precludes complete data on DNA nucleotide sequence, evidence regarding over-all nucleotide composition, the variation of which must reflect gross changes in the nucleotide sequence, is available. Lehman, *et al* [8], have reported the nucleotide composition of DNA produced when primers derived from several different biological sources are added to a system based on polymerase from *E. coli*. If either total *E. coli* DNA, or DNA from the *E. coli*-specific T2 bacteriophage, is used as primer in the *E-coli* polymerase system, the A + T/G + C ratio of the product differs from that of the primer only by 5 and 1 per cent, respectively. However, when the primer is derived from *M. phlei*, the A + T/C + G ratios of primer and product differ by 20 per cent of the primer value; with *A. aerogenes* primer, the difference is 25 per cent; with calf thymus primer the difference is 17 per cent. This is evidence that the relatively precise replication of the primer which occurs in the homologous system depends *both* on the specificity of t[illegible]d on the specificity of the DNA polymerase enzyme.

There is relatively little evidence regarding the possible regulatory effects of the available nucleotides on the biochemical specificity on DNA. *In vitro* experiments show that five-fold variations in the relative proportions of deoxythymidilic or deoxyadenylic acid have no discernible effects on the nucleotide proportions of the product [5]. However, certain *in vivo* experiments suggest that a severe deficiency in deoxythymidilic acid may induce alterations in DNA nucleotide sequence [9].

According to the conventional theory, the ordering of amino acid sequence in the protein is determined by the nucleotide sequence of the "messenger" RNA, a distinctive nucleotide pattern in the latter having an affinity for a particular one of the twenty-odd "transfer" RNA's,

thereby achieving the appropriate positioning of the amino acid uniquely associated with a given tRNA unit. The theory of the DNA code requires that, of the several components of the system which participate in the assembly of the protein's amino acid sequence, only the messenger serves as a source of protein specificity (i.e., its amino acid sequence). If any other source of specificity were to intervene, then the basic conclusion of the theory, that protein specificity (and therefore metabolic control of the cell) is wholly determined by DNA nucleotide sequence, fails. It is appropriate, therefore, to consider the available evidence regarding the origins of the specificity which regulates the ordering of amino acid sequence in protein synthesis.

A crucial test of this question has recently been reported by Barnett and Jacobson [10], who examined the influence of synthetases derived from two different species on the specificity of amino acid attachment to particular tRNA's isolated from one of them. They find that soluble RNA from *E. coli* contains only a single tRNA component which is charged specifically with phenylalanine by the action of *E. coli* synthetase. However, when the synthetase is derived from *N. crassa*, three separable tRNA units found in soluble *E. coli* RNA become charged with phenylalanine: (*a*) One *E. coli* RNA fraction is specifically charged with phenylalanine by both the *E. coli* and *N. crassa* enzyme. (*b*) A second *E. coli* RNA fraction is charged with either phenylalanine or alanine by the *N. crassa* synthetase, but is not affected by the *E. coli* enzyme. (*c*) A third *E. coli* RNA fraction is charged with phenylalanine by the *N. crassa* enzyme, but is not acted upon by the comparable *E. coli* enzyme. The authors regard these observations as evidence for "interspecies degeneracy, ambiguity, and lack of universality in this coding system."

In sum, the present evidence concerning the reactions involved in the development of the specificity finally embodied in proteins does not appear to preclude the participation of sources of specificity *not* derived from DNA nucleotide seq[illegible] and [illegible]t a single gene mutation may give rise to a single ami[illegible] change in a protein are often cited in support of the conclusion [illegible] protein amino acid sequence is completely determined by DNA nucleotide sequence. However, these effects are exhibited only by entire living systems. The only rigorous conclusion that may be derived from such data is that DNA specificity exerts a regulatory effect on amino acid sequence. But no *in vivo* experiment rules out the participation of other sources of specificity, for in all cases the altered DNA is effective only when present in a highly specific cell.

Figure 3B shows how the additional sources of biochemical specificity indicated by the evidence just brought forward would affect the conventional system. DNA specificity is no longer wholly self-determined,

but depends on DNA nucleotide sequence *and* on the specificity of the polymerase (and perhaps on the relative concentrations of free nucleotides as well). Similarly, the specificity of a protein is determined not only by DNA nucleotide sequence (transmitted via "messenger" RNA), but also on the species-specificity of the synthetase.

Accordingly, the systems which govern both the specificity of protein synthesis and of DNA synthesis are fundamentally *circular* in nature. Although protein specificity is derived, in part, from DNA specificity, the latter is, in turn, partially dependent on the protein specificity of the DNA polymerase, which is itself dependent on the specificity of at least one other protein, the amino-acyl RNA synthetase. The system is operative as a self-sufficient source of specificity for DNA and for proteins only if DNA with a specific nucleotide sequence, and at least two proteins which possess species-specificity (polymerase and synthetase) are already present. This situation prevails in the intact cell. It can apparently be imitated with varying degrees of success in *in vitro* systems which derive all of the necessary components from the same biological source, and thereby retain at least some of the complex sources of specificity which are present in the living cells of a given species. As indicated by a comparison of Figures 3A and 3B, the net effect of the introduction of these additional sources of biochemical specificity is to negate the simple conclusion that DNA is by itself a germinal, self-duplicating component or that DNA is the sole source of the biochemical specificity, embodied in proteins, that regulates the specificity of cellular metabolic processes.

A number of experiments show that the introduction of DNA (e.g., bacterial transforming agents) into an appropriate host cell can induce in the latter the appearance of a new operational genetic agent. This result does not conflict with the foregoing conclusion. A given DNA transforming agent is effective only when introduced into a living cell *of the appropriate type*. Hence, replication of the agent may depend on specificity contributed by the host cell.

Nevertheless, it remains true that, thus far, only DNA can serve as an agent of experimental genetic transformation. If, as suggested, proteins share with DNA an ability to determine inheritable biochemical specificity, should it not be possible to achieve genetic transformations with *proteins* rather than with DNA? Yet, such effects have not been observed and it is necessary to account for this fact. The following considerations suggest that this empirical observation may be due to a secondary difference between the status of DNA and of proteins in the cell rather than to any basic difference in their capabilities as regulators of biochemical specificity.

The evidence of genetics shows that DNA agents which are the carriers of typical Mendelian effects are present in the cell in only one or two

replicas; i.e., they are non-redundant. Hence, an invading DNA agent, such as transforming DNA, can readily compete with the DNA-borne genetic agents pre-existing in the host cell, or failing that, appear in an appropriate proportion of subsequent daughter cells unaccompanied by the indigenous agents. Thus, the striking genetic effect of an invading DNA agent is due not only to its regulatory capability, but also to the low level of multiplicity of the cell's normal DNA-borne genetic agents with which it must interact.

However, in the case of a protein, for example, polymerase, matters are very different. A single cell probably contains hundreds or more of separate molecules of a particular enzyme. For this reason, an invading polymerase molecule with a specificity differing from that of the host polymerase, despite its postulated ability to contribute to the specificity of DNA synthesis, is not likely to have a noticeable effect on the biosynthetic activity of the invaded host cell, and little chance of becoming segregated out as the dominant form of the polymerase population of any daughter cells. Hence, no genetic effects are discernible. Nevertheless, such an observation does *not* prove that the polymerase is incapable of contributing to the specificity of DNA synthesis.

Thus, DNA is uniquely capable of transmitting discernible genetic effects because of its unique lack of redundancy in the cell—and not because it is the only molecular constituent of the cell which governs biochemical specificity. Indeed, these considerations suggest that it might be possible to achieve genetic transformations with a *protein* agent—providing that some means could be found for artificially reducing the normally high level of redundancy of proteins in the cell. Such experiments might be possible by previously starving cells, by using inhibitors to reduce the amount of protein which they synthesize, or by using cells that are so small that protein redundancy is sufficiently low to permit the genetic expression of new types of protein experimentally introduced into them.

Non-template Functions of DNA

There is reason to doubt that any germinal system—including the modified scheme just discussed—in which DNA serves as a "template" source of the static specificity of cellular enzymes can fully account for the participation of DNA in the biology of inheritance. The inadequacy of such schemes is suggested by the following considerations among others [11]: (*a*) If inheritable control of metabolism is based only on regulation of the synthesis of particular enzymatic proteins, one can account for qualitative all-or-none effects of typical Mendelian inheritance. However, it is much more difficult to explain the inheritance of quantitative effects on the inheritance of biochemical and biological characteristics without assuming additional regulatory effects. (*b*) A

significant proportion of the DNA in the cells of higher organisms (that contained in heterochromatic portions of the chromosomes) carries exceedingly few or no genes with all-or-none effects of the type which can be accounted for by template mechanisms. Nevertheless, such chromosome segments have important, rather generalized and quantitative genetic effects (see Goldschmidt [12], for a general discussion); their very retention by organisms argues for some biological function. (*c*) Evidence regarding the species-specific DNA contents of different organisms does not conform to the expectation, based on the assumption that each biological character is determined by a DNA template of some fixed size, that more advanced and genetically complex organisms ought to possess correspondingly large cellular DNA contents.

Accordingly, it is useful to consider whether DNA plays some role in biological inheritance, other than providing templates which participate in regulation of protein specificity. Moreover, it will be recalled that the regulatory components operative in living systems include a class of constituents—the free nucleotides—which bear an important relationship to nucleic acids. Free nucleotides serve as precursors in nucleic acid synthesis, so that one may anticipate a possible interaction between the availability of free nucleotides for regulatory roles in metabolism, and the biosynthesis of nucleic acids. These considerations suggest an inquiry into the possibility that free nucleotides are not only regulatory components, but, like the regulatory components already discussed (nucleic acids and proteins), participate in a germinal system. In what follows, evidence is summarized which supports this proposal, and which indicates that this new germinal system can account for genetic effects of DNA that are inexplicable in terms of "template" mechanisms.

(1) As already indicated, various free nucleotides (e.g., ADP/ATP, DPN/DPNH, TPN/TPNH, GTP, CTP) are *regulatory* components with respect to cellular metabolism in that their relative concentrations may determine the choice of alternative metabolic reactions and thus govern the kinetic specificity of cellular metabolism. In addition, it has been shown that the feedback relationship between oxidative electron transport and the ADP/ATP system regulates the species-specific rate of oxidative metabolism in the tissues of laboratory animals [13]. Hence, free nucleotides can be regarded as regulatory components, which determine the choice of metabolic pathway among the numerous alternatives available in the total metabolic system of the cell and the total rate of oxidative metabolism.

(2) The free nucleotides of a cell, including ribonucleotides, are in equilibrium with the system of DNA synthesis, since a number of enzyme reactions are known which permit most of these nucleotides to be converted into the deoxyribonucleotides that are the direct precursors of DNA. There is direct evidence that inhibition of DNA synthesis,

for example, by irradiation or by antibiotics, causes marked increases in intra-cellular nucleotide levels [14]. In certain embryos, sharp changes in nucleotide levels have been observed during periods of rapid DNA synthesis [15]. Moreover, the bulk of the DNA in the cell appears to be extraordinarily stable. For these reasons, the synthesis of DNA may be

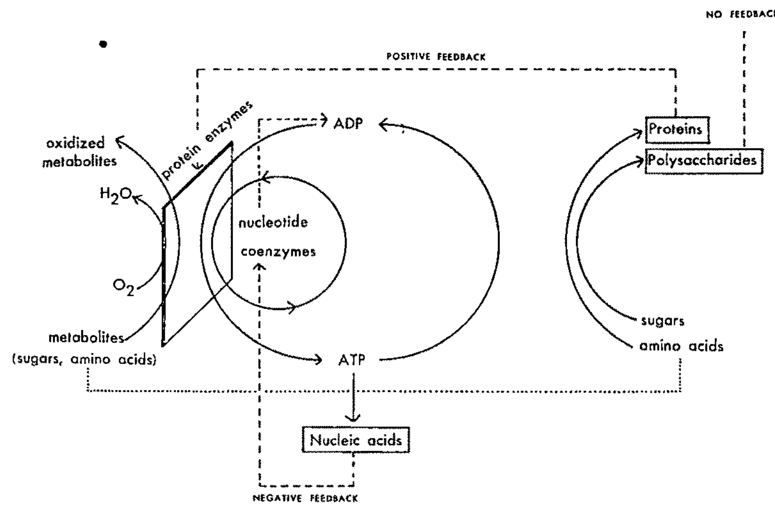

FIG. 4. Diagram to illustrate inter-relationships between oxidative metabolism, and the synthesis of polysaccharides, proteins and nucleic acids respectively. The left hand part of the diagram symbolizes the activity of the enzyme system of oxidative metabolism, enzymes, generically, being represented by the vertical surface. Synthesis of polysaccharides and proteins is coupled to oxidative metabolism by the ADP/ATP system; in both cases, synthesis of the polymer requires ATP, but ADP is generated in the process. Since ADP concentration regulates the rate of oxidative metabolism, which in turn determines the ATP level, the ADP/ATP balance is maintained when these polymers are synthesized. Protein synthesis has a positive feedback relationship to oxidative metabolism, since the enzymes necessary for the latter are proteins. In contrast, nucleic acid synthesis not only depends on ATP (and other nucleotides), but also results in the sequestration of the entire nucleotide residue. Hence, oxidative metabolism is coupled to nucleic acid synthesis by a negative feedback relationship. Therefore, synthesis of a very stable nucleic acid, especially DNA, may be expected to lower the levels of free nucleotides, and with it the rate of oxidative metabolism characteristic of a specific cell.

regarded as an effective mechanism for the sequestration of free nucleotides which would otherwise be active in cellular metabolic processes. Hence, DNA synthesis will tend to decelerate the oxidative processes on which it depends; in this respect DNA synthesis has a unique relationship to oxidative metabolism. (See Fig. 4.) Even if the effect of DNA synthesis on the free nucleotide levels is transitory, it is likely to result in lasting changes in the metabolic pattern of the cell, for example, by feedback inhibitions mediated by the free nucleotides. Hence, we may

propose that the amount of DNA synthesized in the lifetime of a cell will determine the intracellular levels of free nucleotides, at least during some period of cell development, which will in turn exert a lasting effect on the cell's pattern of metabolic pathways, and on its characteristic rate of oxidative metabolism.

(3) The amount of DNA found in most cells is a fixed species-specific characteristic, so that the amount of DNA synthesized in the life history of the cell is also fixed. This fact, together with those summarized above, leads to the following: DNA/cell $\Longrightarrow$ amount of DNA synthesized/cell $\Longrightarrow$ free nucleotide level $\Longrightarrow$ rate of oxidative metabolism and choice of metabolic pathway. Since DNA is carried by the chromosomes, which are regularly transmitted in inheritance, the foregoing relationships provide a genetic mechanism. This mechanism is based on the *amount* of DNA synthesized in the cell (and perhaps secondarily on the relative nucleotide composition of the DNA) and has no direct relation to its nucleotide sequence. Hence, the proposed genetic system is quite distinct from genetic mechanisms based on the "template" function of DNA.

(4) Accordingly, DNA has a dual role in inheritance. The conventional scheme, as modified earlier, proposes that DNA nucleotide sequence in part determines what specific enzymes are present in the cell, and thereby participates in a precise regulation of cellular metabolism. The hypothesis of nucleotide sequestration proposes that, *in addition*, the mere synthesis of DNA, without regard to nucleotide sequence, determines a choice of metabolic processes which is mediated by intracellular concentrations of free nucleotides. The sequestration system will affect rather general metabolic characteristics, such as the balance between aerobic and fermentative systems, the total rate of oxidative metabolism, and over-all cell size (which is generally inversely related to metabolic rate). It can be regarded as a kind of coarse adjustment of cellular metabolism, on which are superimposed the more limited but precise effects of the template system.

(5) The data of biological inheritance support the proposal of a dual effect of DNA on inheritance. In the cells or higher organisms, chromosomal DNA appears in two forms, euchromatin and heterochromatin. Euchromatin is the locus of the typical Mendelian genes which affect specific, limited processes in an all-or-none fashion. These genes appear to be mediated by the action of specific enzymes, so that their effect is accountable by the regulation of protein structure by way of a DNA "template," although the limitations on the "code" theory already mentioned should be kept in mind. On the other hand, heterochromatin does not appear to contain typical Mendelian genes. Nevertheless, heterochromatin does have certain genetic effects [16], and these can be correlated with the proposal of DNA sequestration of free nucleotides. (*a*) Heterochromatic genetic effects are usually quantitative rather than

all-or-none. According to the sequestration hypothesis, free nucleotide levels—and consequently the cell characteristics influenced by these levels—are expected to vary inversely with the amount of DNA synthesized, and therefore to exhibit graded rather than qualitative effects. (*b*) Heterochromatic genetic effects are quite generalized and especially influence cell size, metabolic rate, and growth rate. These are features mediated by free nucleotide levels and thereby, according to the proposal, by DNA synthesis. (*c*) Heterochromatic segments tend to induce a high rate of mutation in nearby euchromatic genes. The effect diminishes with distance, and therefore may be governed by a diffusion process. According to the sequestration hypothesis we may expect synthesis of local heterochromatic DNA to influence the availability of nucleotides for synthesis of DNA at the sites of nearby euchromatic genes. Such a process might be expected to influence DNA nucleotide sequence and therefore the incidence of mutations in euchromatic genes.

(6) The genetic role of DNA in the sequestration system is not autonomous. While the characteristic amount of DNA synthesized in a cell appears to be influenced by the amount of DNA pre-existing in the cell, DNA synthesis is also likely to be regulated by the state of the feedback system connecting the free nucleotides (especially ADP) to the rate of oxidative metabolism. Oxidative metabolism is largely localized in the mitochondria, which are self-perpetuated cellular elements subject to internal mutations that have transmissible effects on the levels of oxidative metabolism. Hence, in the over-all system, at least one element, the rate of oxidative metabolism, cannot be regarded as exclusively determined by DNA, although DNA synthesis depends on it. Therefore, the germinal attributes of the system are, at the least, the joint properties of the amount of pre-existing DNA and the cellular rate of oxidative metabolism. No single component of the system can be regarded as "self-duplicating."

(7) The germinal properties of the two DNA systems are interdependent; neither one is alone capable of complete regulation of the phenotypic effects with which it is associated. The abundant observations of heterochromatin-induced mutations of euchromatic genes indicate that the precision with which the DNA of these genes can be replicated is influenced by the nature of nearby heterochromatin. The genetic effects of the heterochromatic system appear to be the consequence of DNA synthesis *per se*. Since euchromatic DNA is an equally effective agent of nucleotide sequestration, it is logical to conclude that the effect of heterochromatic DNA synthesis on the free nucleotide pattern will be influenced by concurrent synthesis of euchromatic DNA. Nor can the total complex of DNA-mediated inheritance be regarded as autonomous, or capable of "self-duplication." The sequestration system, and therefore both DNA systems, depends on at least one self-per-

petuated mutable cytoplasmic agent, the mitochondrion, and other cytoplasmic agents of this type may contribute to the determinative effects of metabolism on the DNA as well. This is to say, of course, that the simplest system capable of self-sufficient germinal activity and of actual self-duplication is the entire living cell.

The foregoing proposals are subject to a certain degree of experimental verification at this time. If they are correct, one should expect to find these otherwise unexpected relationships: (*a*) The species-specific DNA content of an organism's cells should bear an inverse relationship

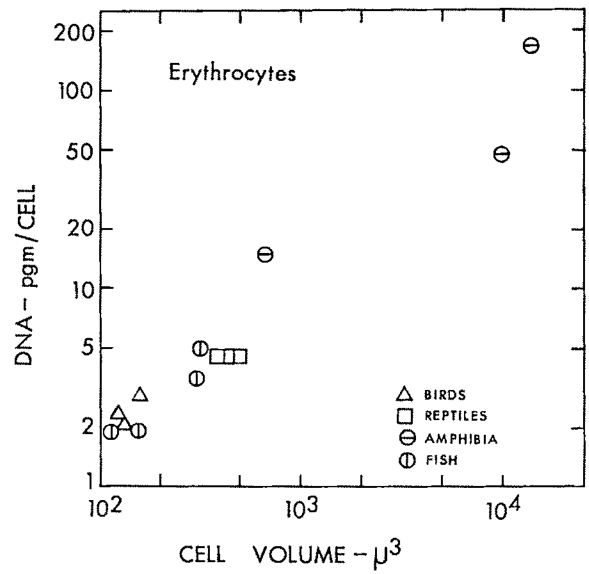

FIG. 5. Relationship between cell size of erythrocytes of various vertebrate genera and the organism's characteristic cellular DNA content. Figure reproduced from Reference [17], which should be consulted for details and relevant references.

to the species' characteristic rate of oxidative metabolism (e.g., QO_2). (*b*) DNA/cell ought to be directly related to the species' characteristic cell size.

Certain data available in the literature provide a test of these conclusions [17]. These are shown in Figures 5, 6, 7, and 8 and in Table 1. Figure 5 shows that, over a wide range of values, the characteristic cell size of nucleated erythrocytes in a series of vertebrate species is proportional to the value of DNA/cell. Figure 6 shows that, in the case of the ten species for which the necessary values are available, the erythrocyte's characteristic rate of oxygen consumption is inversely proportional to the value of DNA/cell. Figure 7 shows that a similar relationship between DNA/cell and basal metabolic rate of the entire organism

is observable in the case of a series of mammals, birds, and reptiles. Finally, Table 1 shows that the proposed relationship between DNA/cell

TABLE 1

THE CELLULAR DNA CONTENTS OF VARIOUS AMPHIBIA

Genus	*Metamorphic Type*	*DNA, Pgram/Cell*	*Reference*
Amphiuma	Neoteny universal	168	(*a*)
Siredon (axolotl)	Neoteny in some individuals	96	(*b*)
Necturus	Neoteny universal	48.4	(*a*)
Triturus (viridescens)	Neoteny in some individuals	98	(*c*)
Rana	Metamorphosis obligate	15	(*a*)
Bufo	Metamorphosis obligate	7	(*a*)

References: (*a*) Mirsky and Ris [18]; (*b*) S. Løvtrup, *J. Expt. Zool.*, *141*, 545 (1959); (*c*) H. Swift in "The Chemical Basis of Development," W. D. McElroy and B. Glass, ed., The Johns Hopkins University Press, Baltimore, 1958.

and metabolic rate may elucidate the hitherto unexplained occurrence of excessive cellular DNA contents in certain amphibia. Amphibia, such as frog and toad, which undergo metamorphosis (so that the respiratory rate of the adult is increased over that of the larva) exhibit values of DNA/cell which are not very different from those of other vertebrate species (15 and 7 pgram DNA/cell respectively). However, those amphibia (such as *Amphiuma*) in which neoteny—a condition in which metamorphosis and its attendant increase in metabolic rate does not occur—contain very high cellular DNA contents (168 pgram of DNA/cell in the case of *Amphiuma*). Amphibia in which neotenic individuals are found with a frequency which depends on the habitat region also exhibit high values of DNA/cell. Thus, excessive cellular DNA contents appear to be associated with a species characteristic—neoteny—which results in a very low metabolic rate in the adult. The same effect may account for the excessive DNA content of the lungfish (100 pgram/cell), which has a low metabolic rate and sluggish habit associated with the presence of lungs but the absence of terrestial habit.

Bacterial cells contain DNA in amounts considerably below that found in the cells of higher organisms, and this is often regarded as evidence that the more complex organisms contain a greater number of "template" genes and therefore more DNA. A possible alternative is that differences in DNA content are related to the sequestration system rather than to the "template" system. Although the considerable variability of bacterial metabolic rates precludes a direct examination of this question, it is possible to test the relationship between cell size and DNA/cell. Figure 8 shows that the values for a series of bacterial cells fall quite well on the line established by the relationship between cell size and DNA/cell

of nucleated erythrocytes, although the bacterial values are 2–4 orders of magnitude less than those of the erythrocytes. This suggests that the major factor which determines the DNA contents of cells of species as widely different as bacteria and vertebrates is the DNA sequestration system rather than the number of "template" genes.

When it was first discovered that DNA content per cell is a species-

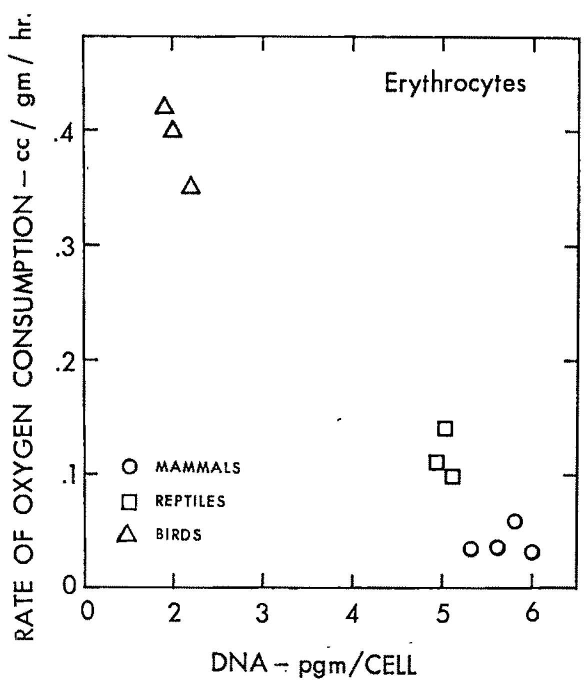

FIG. 6. Relationship between the rate of oxygen consumption of various vertebrate erythrocytes and the characteristic diploid cellular DNA content of the organism. Figure reproduced from Reference [17], which should be consulted for details and relavant references.

specific characteristic, an intensive effort was made to elucidate the relationship between this parameter and the species' evolutionary position [18]. The results were disappointing, when viewed in terms of the expectation, based on the template function of DNA, that advanced organisms, being complex, should contain correspondingly large numbers of genes and cellular DNA contents. However, as illustrated in Figure 6, the relationship between DNA/cell and metabolic rate leads to a clear taxonomic separation between the DNA contents of mammals, as compared to birds and reptiles. It is probably also significant that birds and reptiles, which are rather closely related, appear to form a

single function with respect to DNA/cell and metabolic rate. Finally, when the influence of metabolic rate is taken into account, the difference in DNA content between the more advanced group, mammals, and the less-advanced bird-reptile group that is expected from the template theory does in fact emerge. When compared *at a given metabolic rate*, the mammalian species exhibit a cellular DNA content 2–3 pgram

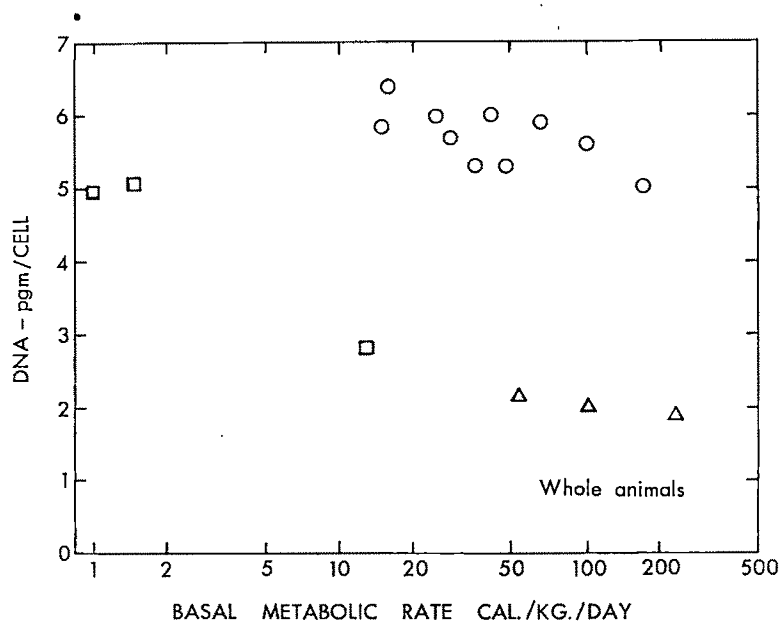

FIG. 7. Relation between basal metabolic rates of various vertebrate genera and the organisms' characteristic cellular DNA content. Figure reproduced from Reference [17], which should be consulted for details and relevant references. Circles, mammals; squares, reptiles; triangles, birds.

greater than species in the bird-reptile group. This suggests that both systems are operative in determining species-specific DNA content.

Some Implications

The chief conclusion to be derived from the foregoing considerations is that the unique capability of living organisms for self-duplication and inheritance arises from complex multi-molecular interactions among at least several classes of cellular components. Neither DNA nor any other cellular component is a "self-duplicating molecule" or the "master chemical of the cell"—terms which sometimes appear in current generalizations. There is no evidence from recent investigations of the biochemical aspects of genetics which requires abandonment of the con-

clusion, long established by biological data, that the least complex agent capable of self-duplication is the intact living cell.

The point of view developed here also suggests alternatives to a number of current views of phenomena related to inheritance. (*a*) Various hypothetical schemes for regulating the "activity" of template genes are invoked in order to explain how the DNA code might operate in cellular differentiation and development. Alternatively, it may be

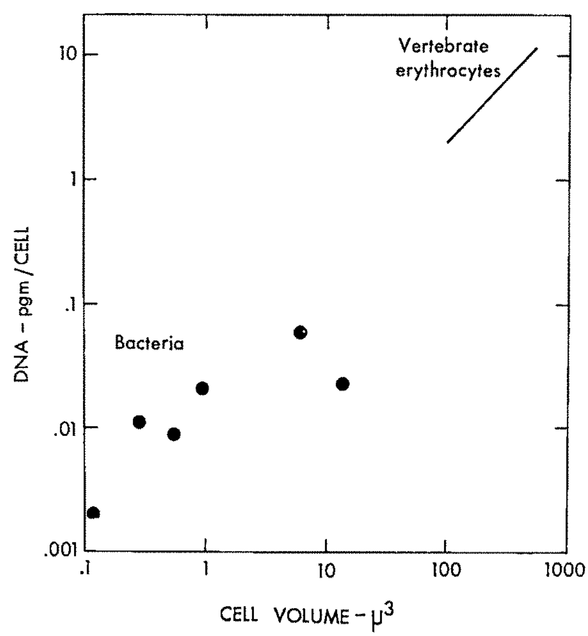

FIG. 8. Relationship between cell volume and cellular DNA content for a series of bacteria compared with similar data for vertebrate erythrocytes. The line summarizes the values for various vertebrate erythrocytes shown in Figure 5. Figure reproduced from Reference [17], which should be consulted for details and relevant references.

suggested that differentiation is associated with the nucleotide sequestration system, tissue-specific changes in DNA synthesis (e.g., at chromosome "puffs") regulating nucleotide levels and thereby governing the size and metabolic character of the cell. (*b*) The relationship between DNA and a species' characteristic sensitivity to ionizing radiation is often interpreted in terms of radiation-induced mutations in genetic templates. Alternatively, radiation-induced inhibition of DNA synthesis, which results in the accumulation of toxic concentrations of free nucleotides, may be regarded as the mediating process. This view is compatible with Sparrow's [19] observation that in certain non-poly-

ploid plants, radiation sensitivity is proportional to cellular DNA content, and is particularly affected by the relative proportions of heterochromatin. (*c*) Sahasrabudhe [20] has suggested that tumor cells may be characterized by marked changes in free nucleotide level resulting from nucleotide sequestration due to DNA synthesis. In this connection, it is also of interest that tumor and other rapidly growing cells exhibit distinctive pathways of oxidative metabolism, which are in turn regulated by free nucleotide levels. (*d*) The theory of the DNA code suggests a seemingly simple explanation for the emergence of the first forms of life from the prebiotic organic environment: that life began with the fortuitous appearance of a "self-duplicating nucleic acid molecule" which then organized the complex chemistry of life around itself [21]. In contrast, the viewpoint developed here suggests that the primitive function of nucleic acid was to sequester free nucleotides, and thereby regulate over-all metabolic activity. Thus, the nucleic acid template is to be regarded as a late development which improved the precision of the earlier modes of regulation of cellular activity, and which is, in any case, incapable of self-duplication.

The theory of the DNA code, if correct, also leads to important expectations regarding the feasibility of technological control over inheritance. If the nucleotide sequence of DNA were indeed a self-sufficient source of the inherited specificity of living things, then it might be possible, in the not too distant future, to synthesize artificial DNA molecules, which, on being introduced into living organisms, would artificially establish new inheritable characteristics quite outside the range of those normally observed. However, if, as we have concluded, biological specificity is only partly due to DNA, then it is likely that the specificity represented by proteins may impose severe limits on the acceptability, to the cell, of abnormal DNA nucleotide sequences. This view suggests that technological control over biological inheritance may be far less plausible than indicated by inferences drawn from the code theory.

Finally, the viewpoint developed here bears on some fundamental questions regarding the relationship between physical theory and biology. The theory of the DNA code is often regarded as an example of the success with which "modern physics" can solve basic problems about living systems which have eluded the supposedly less critical analyses of classical biologists. This view leads to the generalization that, despite its obvious complexity, the living cell must be governed by the "laws of physics"—as revealed by analysis of non-living systems—and that the most effective strategy for elucidating its unique properties is to study isolated parts which are sufficiently simple to permit their analyses in physico-chemical terms. However, several developments in theoretical physics suggest that the fundamental properties of matter are in better harmony with the view that the unique properties of the cell are derived

from interactions of its molecular parts and are inherently incapable of being elucidated by a simple summation of the observed properties of the isolated parts. This viewpoint is a direct outgrowth of Bohr's theory of complementarity [22]. It may also be closely related to the more recent theory of the "Bootstrap Universe" [23]. This theory suggests that the unique properties of the atomic nucleus are inherently associated with its complexity, and are not accountable by the observed properties of fragments isolated from disrupted nuclei.

It may be suggested then, that, if biology is to be guided by the insights into the properties of matter that are afforded by modern physical theory, the role of DNA in the living cell must be viewed as subsumed under the complex properties of the system—the living cell—of which functional DNA is a part. The theory of the DNA code is sometimes epitomized by the statement "DNA is the secret of life," an aphorism which appears increasingly to guide the course of current biological investigations. The viewpoint developed here suggests that biology might be more wisely guided by the aphorism, "Life is the secret of DNA."

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AMERICAN SCIENTIST 52, 1964

PERSPECTIVES

AN ESSAY REVIEW

By AARON LEMONICK

The Encyclopaedic Dictionary of Physics, edited by J. THEWLIS, *et al.;* Volumes 1 to 8; $298; 2 part glossary gratis with Volumes 1 to 8, $60 if purchased separately; The Macmillan Co., 1963.

AT ONE point in my life I must have believed that an encyclopaedia contained all knowledge for I can still remember the feeling of awe at the very idea of encyclopaedia. I believe that I thought there was no topic on which the encyclopaedia was not the complete and final authority. I may even once have planned to read through the entire encyclopaedia some day. I no longer have the urge nor the illusions.

Now here I am reviewing the *Encyclopaedic Dictionary of Physics* recently published by the Macmillian Company. I confess that I approached this task with great misgiving. I know how to begin to review a book. I read it. In the case of the set of eight books under review the task is so different, so distorted by its very size, that the difference is not only quantitative—which it certainly is—but qualitative as well. Of the eight volumes one contains a subject and an author index. It seems to be a good index with lots of entries and cross references. I won't say any more about it. Perhaps I can convey the scope and size of the remaining seven volumes by some statistics.

Each of the seven volumes has about eight hundred pages, or five thousand and six hundred pages in the entire set. Each page contains about one thousand words, making a total of over five million words. The articles themselves vary in size, but only the exceptional article is over two to three thousand words, according to the editor. The shortest articles are about a line or two. The length of the majority of the articles is, however, neither of these extremes—the average article seems to be several hundred words in length. That would say that there are over ten thousand articles (which checks roughly with a cross-referenced index which has fifty thousand entries). According to the author index, these ten thousand articles were written by over fifteen hundred contributors, all but a few of whom are from Great Britain or the United States. J. Thewlis of the Atomic Energy Research Establishment at Harwell in England, the Editor-in-Chief of the *Encyclopaedic Dictionary*, discusses the coverage of the set—"This present Dictionary aims, therefore, at covering not only physics proper but also, to a greater or lesser extent

such subjects as mathematics, astronomy, aerodynamics, hydraulics, geophysics, meteorology, physical metallurgy, radiation chemistry, structural chemistry, crystallography, medical physics, biophysics, and photography." Here is a list of the sixty-seven topics which formed the framework of the Encyclopaedia.

Acoustics
Astronomy
Astrophysics
Atomic and molecular beams
Atomic and nuclear structure
Biophysics
Cathode rays
Chemical analysis
Chemical reactions, phenomena, and processes
Chemical substances
Colloids
Cosmic rays
Counters and discharge tubes
Crystallography
Dielectrics
Elasticity and strength of materials
Electrical conduction and currents
Electrical discharges
Electrical measurements
Electrochemistry
Electromagnetism and electrodynamics
Electrostatics
Engineering metrology
General mechanics
Geodesy
Geomagnetism
Geophysics
Heat
Hospital and medical physics
Industrial processes
Ionization
Isotopes
Laboratory apparatus
Low-temperature physics
Magnetic effects
Magnetism
Mathematics
Mechanics of fluids
Mechanics of gases
Mechanics of solids
Mesons
Meteorology
Molecular structure
Molecular theory of gases
Molecular theory of liquids
Neutron physics
Nuclear reactions
Optics
Particle accelerators
Phase equilibria
Photochemistry and radiation chemistry
Photography
Physical metallurgy
Physical metrology
Positive rays
Radar
Radiation
Radioactivity
Reactor physics
Rheology
Solid-state theory
Spectra
Structure of solids
Thermionics
Thermodynamics
Vacuum Physics
X rays

The Editor goes on to say that the set should be useful not only to physicists and would-be physicists, but also to scientists who are con-

cerned with fields mentioned or implied in the preceding list which have a physical basis.

You will, I think, agree that there are difficulties in reviewing a piece of work which is as large and which has as broad a coverage as this Encyclopaedic Dictionary. Perhaps I should have started at the beginning and read it through. I would have been surer that I hadn't overlooked something—that I wasn't, by chance, reporting on a biased sample. Rather, in order to get a feeling for the book as a whole I tried three different attacks. One was to put the set on my desk for a period of time and use it to look up words that came up in the normal course of my reading. The second was to open pages at random and read the articles on that page. Third, I followed up two fields with which I am familiar through the indexes and cross references.

The articles seem to be written at a level which would make them accessible to advanced undergraduates or beginning graduate students in physics. The articles on theoretical physics and mathematics are somewhat more advanced. They all seem well-written and well-illustrated.

Perhaps I can best give you a feeling for the books by writing about several articles in some detail, listing a set of articles within a field to show the coverage; listing a set of articles related only by their juxtaposition, and then telling you a little about where I differ from the editors and contributors in matters of scope and emphasis of the articles and the selection.

Take as an example the article on the Fundamental Particles. It is about fifteen hundred words in length with two tables, a table of the properties of the thirty particles then known and one of the decay modes of the unstable particles. After first defining the concept of elementary particles, the article goes on to tell of the parameters that characterize the particles. The article then gives the energy thresholds for production and tells of the conserved quantities. A table lists the particles, their mass, spin, isotopic spin, strangeness, and mean life. The bibliography contains five references to articles and books by such physicists as Dalitz, Gell-Mann, Rosenfeld, and Jackson. The article is a reasonable first stop for someone who knows very little about the field and wants to learn a little or learn where he can learn much more. The references are, perhaps by design, probably far beyond the level of anyone who has to look in the Dictionary for the basic facts about the elementary particles.

In the article on the Detection of the Fundamental Particles nearly all the methods for detecting the elementary particles are mentioned in the context of the description of the original discovery and identification of the elementary particles. The article is a good introduction to the history of the discovery of the particles. I have never before seen all this information gathered together so conveniently. This article is about two and a half pages long or about twenty-five hundred words. I was

interested to note that the entries on furnaces, graphite tube; furnaces, industrial; furnaces, laboratory; furnaces solar, take almost ten full pages or ten thousand words.

The article on Quantum Mechanics is a two thousand word article which gives a good summary of the high points. It is not for the casual reader. It would take someone who knew more than is in the article to be able to understand the article. The bibliography is short but contains very good references. They are all, with one exception, full treatments of Quantum Mechanics at an advanced level. I am troubled by the mismatch of levels between the reader likely to look up Quantum Mechanics in this article, the article, and the bibliography.

Nuclear physics is a field which is probably familiar to more people than most other fields so it seems to me to be a good place to examine the coverage of the Dictionary. The "Nuclear" articles take about one hundred twenty pages and are very well done. Here is a list of these articles—Nuclear Adiabatic Demagnetization; Nuclear Capture of Particles; Nuclear Charge, Effective; Nuclear Collisions, Particle Production In; Nuclear Disintegration; Nuclear Emulsion (4 articles); Nuclear Energy; Nuclear Energy Levels (3 articles); Nuclear Evaporation; Nuclear Ferromagnetism; Nuclear Fission (4 articles); Nuclear Forces (4 articles); Nuclear Fuel, Chemical Processing of; Nuclear Induction; Nuclear Magnetic Resonance; Nuclear Magnetism; Nuclear Paramagnetism; Nuclear Magneton; Nuclear Matter, Homogeneous; Nuclear Models; Nuclear Moments; Nuclear Photoelectric Effect; Nuclear Photomagnetic Effect; Nuclear Physics; Nuclear Pile (2 articles); Nuclear Potential Well; Nuclear Propulsion; Nuclear Radius; Nuclear Reactions (5 articles); Nuclear Reactor (87 articles; 56 pages); Nuclear Relaxation; Nuclear Resonance Integral; Nuclear Resonance Level; Nuclear Rotation, Tidal Wave Type Of; Nuclear Scattering; Nuclear Shape; Nuclear Shell Structure; Nuclear Spectroscopy; Nuclear Spin; Nuclear Spin Echoes; Nuclear Stability; Nuclear Statistics; Nuclear Surface; Nuclear Temperature.

To relieve the nuclear list I picked another part of the same volume at random where you can find the following variety of subjects:—Photo Voltaic Effect; Physical Constants; Physical Medicine; Physics; Pi Meson; Pinch Effect; Pipe Flow; Pitch; Pilot Static Tube; Planck's Radiation Formula, Plane; Plane of Weakness, Geological; Planet.

My difference with the editors is a matter of taste. I believe that the Dictionary is rather heavy on technology. The longest articles seem, on the whole, to be technological ones. There is certainly nothing wrong with long articles, but in an Encyclopaedic Dictionary of Physics I would expect them to be physics articles. With the average length of the articles being what they are, I find it surprising that Air Conditioning gets four full pages, as does Photographic Darkrooms. Solid Propellants

is given five full pages. No physics article I read was this long. Gauge Invariance gets seven lines, but gauges of one sort or another get thirteen pages.

I also found some peculiar omissions. There may be others, but I can only report on the ones I noticed. Perhaps I saw the only two in the Dictionary. The Eötvös Rule is mentioned, but not the Eötvös Experiment—a fundamental experiment in the theory of gravity. Opalescence is written up as is Critical Point, but nowhere could I find reference to Critical Opalescence and its evidence for giant statistical fluctuations.

This Encyclopaedic Dictionary has faults, but these can be combed out in future editions. However, the Editor J. Thewlis and his Associate Editors R. C. Glass, D. J. Hughes (now deceased), and A. R. Meetham and the Publishers deserve the greatest credit for accomplishing a major work. There is really nothing to compare with it in reference books for physics. There is a great gap between the Handbuch, with each article a definitive review article, and the one volume dictionary. I expect that this Dictionary will be of use to the people for whom it is intended—graduate and pre-graduate students in Physics and specialists in other fields. The volumes will be of much less use to physicists.

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(The actual investigator will as always be available for the bull sessions that justify the travel expense. The thespian *alter ego* cannot be expected to field hard questions.)

Where management does not yet provide the full service, a temporary expedient worthy of consideration by the non-professional presenter is to request the free pamphlet "Effective Lecture Slides" from Eastman Kodak Company, Sales Service Division, Rochester, N.Y. 14650. The management itself may even appreciate knowing that it can order this pamphlet in lots of 100 at \$6 per 100.

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Most IRTRAN material and most work of infrared opticists wind up in military hands. The security curtain is rather opaque. We are free to issue data within the U.S.A. on the physical nature of the materials. Beyond that we play dumb. Apart from IR technology for the analytical lab and chemical process control (where all is well), we do not envy those who must decide what may safely be disclosed.

Military considerations ultimately and inevitably have emotional content. Emotion suffuses the idea that someone who means you harm has technical devices for extending the senses that he and you both received at birth. You hope you know more about these means than he does and more about what to look for with these means. To feel this way goes along with the human condition. It sets one limit on the decisions of the declassifying boards.

The opposite limit is set by another realization which the members of these boards share with you. They too know that secrets stale quickly and that scientists in a position to freshen them up were born with a "need to know" but can produce no documents to prove it and often don't even want to. Intelligent military seek an optimum between these limits.

Two unclassified symposia on "Remote Sensing of Environment" and one classified one at the Institute of Science and Technology of the University of Michigan have given the military the thoughts of such diverse types as geologists, botanists, astronomers, conservationists, meteorologists, oceanographers, foresters, archaeologists, and half a dozen species of engineers. Everybody present knew that the already known applications will take care of themselves but wondered about the unknown ones that are in danger of dying unborn for want of cross-fertilization.

The final report of the symposia, identified as Document 4864-6-F, carries the following statement: "Qualified requesters may obtain copies of this document from: Defense Documentation Center, Cameron Station, Alexandria, Virginia." We have inquired how one qualifies as a requester and have been informed by the Defense Documentation Center that all reports it releases must be for use on a current Department of Defense contract or project.

THE SCIENTISTS' BOOKSHELF

By Hugh Taylor, the Associate Editors, and Guest Reviewers

SEE INDEX AT END OF THIS SECTION

Masers & Lasers—How They Work, What They Do by M. BROTHERTON; 207 pages; $8.50; McGraw-Hill Book Co., 1964.

This is a wonderful book, in the sense that it records the astonishing developments in one of the most sophisticated areas of modern physics and the tremendous possibilities in technology that have followed from "pure" research which began in Columbia University in 1954, just ten years ago. The basic science goes back further, to a fundamental discussion by Einstein of what happens when radiation exists in a state of equilibrium with matter. Einstein's analysis distinguished three phenomena, absorption of radiation by matter, spontaneous emission, and stimulated emission. In Chapter 7, after the concepts pertinent to the new problem have already been lucidly exposed, the author describes the first maser and the background in which it was created. He discusses that perennial problem in modern scientific progress: why the discovery had not come earlier, say in the 1930's, after Einstein's basic paper. From Townes' original ammonia gas maser, through the traveling ruby maser "which so brilliantly performed in the Echo and Telstar experiments," the author traces the evolution of the maser into the laser, from *m*icrowave *a*mplification by *s*timulated *e*mission of *r*adiation, to *l*ight *a*mplification of the *s*timulated *e*mission of *r*adiation.

The book "was conceived for that considerable audience of literate people . . . who would like to have an explanation of masers and lasers which, while factual and authentic, stays away from formulae and formulations beyond their grasp." A brilliant success has been achieved. Chapter 9 on Ruby Lasers and Helium-Neon Lasers, Chapter 10 on Semi-Conductor Junction Lasers, Chapter 11, Waves and Wave Motions, in which, starting from the observation of wave properties "by watching water waves as we sit in a rowboat gently rocked by a ground swell" and the consequences of a speedboat going bouncing by, the literate reader finds himself accepting easily Fourier's theorem of complex vibrations reducible to simple sine waves. The final chapters reveal why the laser's output can be at the same time intense, coherent, and extremely directional, with pulse intensities equivalent to a temperature of 10 million billion °C. compared with the sun's surface temperature of 6000°C. and directional precision "applicable in surgery to eliminate even one cell from an infected area and, in cases of detached retina, to 'weld' the retina back in position."

The reviewer has only one regret. The "literate people," outside the scientists and engineers, who need to supplement their humanistic knowledge with such authentic and fascinating treatment of new science will unwillingly pay the price of this book. Should not another effort be made to bring it to them?

The Story of the Laser by JOHN M. CARROLL; 181 pages; $3.95; Dutton & Co., 1964.

This book covers the same ground as that of the preceding review, and at one-half of the price to the reader. It contrasts in style with the book by Brotherton. Its non-technical presentation is interspersed with anecdotes and personal notes concerning the scientists engaged in the development. The early morning meditations of Charles H. Townes in the gray Washington dawn, in Franklin Park, admiring the azaleas, then at the height of their

bloom, led to calculations, "on the usual back of an envelope, (of) the critical condition for oscillation in terms of the number of excited molecules which must be supplied and the maximum losses allowable in the cavity." This was the beginning of the ammonia laser. Such interpolations, some excellent photo-reproductions of places, persons, and experiments in the laser field, diagrams illustrating the text and a chapter on "Lasers in War and Peace" serve to characterize this particular story of the newest development in science.

Noble Gases and Their Compounds by G. J. Moody & J. D. R. Thomas; 62 pages; $2; The Macmillan Co., Pergamon Press, 1964.

This is a record in one small volume, at a reasonable price, of all the work on over twenty compounds of the noble gases which have been produced since Bartlett published, in June 1962, his first report of an authentic compound of a noble gas, xenon hexafluoro platinate (V). A résumé of the discovery of the noble gases and early efforts at inducing chemical combination precedes the record of the successful achievement of compound formation. Eleven references deal with XeF_2, seven with XeF_4, and five with XeF_6. There are xenon oxides and oxyfluorides, xenates, and a few compounds of krypton and radon. These researches indicate that the concept of electron-filled shells in the rare gases has been over-emphasized and that the word "inert" applied to such gases is now no longer valid. The book records a thrilling year of research in inorganic chemistry.

Concepts in Solids by P. W. Anderson; 188 pages; $8.50 cloth; $4.95 paper; W. A. Benjamin, Inc., 1963.

This book is indeed aptly titled. Dr. Anderson here describes the concepts and attitudes which have developed in Solid State Physics in the course of the last twenty years. His approach is quite distinctive and personal, but it exemplifies the best aspects of the new approach in the theory of solids.

Because of the emphasis on newer material and newer points of view, this book is certainly not a balanced presentation of theoretical solid state physics. Rather, "Concepts in Solids," is an elegant and deep presentation of a few selected topics. The presentation is designed to give a rather sophisticated audience—graduate students and research workers—a deeper insight into the nature of the solid state.

The topics chosen do, in fact, reveal the core of modern solid state theory. There is a natural division into one-electron theory and many-body theory. One electron topics include discussions of methods of calculating band structure both with and without external perturbing fields. The treatment of the Hartree-Fock method and of effective Hamiltonian theory are particularly illuminating. The second half of the book is devoted to the concepts of elementary excitations in many-body theory. The discussions of broken symmetries and of the magnetic state are particularly unique and particularly useful.

This book grew out of a series of lectures at Cambridge University. The informal format and editing are quite appropriate for the publication of a set of lecture notes.

The price of the clothbound version, $8.50, is too high for a book of 188 pages, but the elegance of the contents makes this book invaluable for a serious student of the solid state.—*Leo P. Kadanoff*

Unusual Environments & Human Behavior, Physiological & Psychological Problems of Man in Space, edited by Neal M. Burns, *et al.;* 438 pages; $9.95; The Free Press of Glencoe, 1963.

This volume follows what is a growing fashion in books on space; it is a collection of review papers: twelve chapters and sixteen authors. The subject matter deals with the study of human responses to stress, particularly the environmental stresses which may be met in manned space flight. The purpose of the book is to "describe the ranges of these environmental variables that enable us to categorize them as

stressful or unusual and to arrange the data concerning these variables into a presentation that describes the effects of exposure to them." There is also a plea by the editors for an increase in research on methods of extending human tolerance limits, research on combined stress and on improved structure for this entire field of research.

The book is divided into two parts, "Part I, Overviews" and "Part II, Specific Problem Areas," in a partially successful attempt to cast the field into perspective before examining in detail the work which has been done in a number of specialized areas.

The quality of the papers is quite variable; as a result, a number of problems central to the subject are treated only lightly, if at all. Examples of neglected subjects are: the problems of earth-based predesign system simulation using human subjects, the need for and problems of providing artificial gravity in space flight, the metabolic changes which are brought about by unusual environments and the effects of closed environments of unusual atmospheric composition. Three of the chapters in Part I—those of Lawton, Ruff and Weybrew—are very good. The chapter, "Psychophysiology of High Altitude Experience" is better suited to Part II and the chapter "Computor Simulation of Man-Machine Systems" is such a detailed discussion of a single concept in a rather large field that one questions the wisdom of its inclusion in a book of this scope and length.

References are placed at the end of each chapter and are as variable in value as the papers. There is a disturbing preponderance of Air Force publication references in those chapters prepared by Air Force officers and employees, and similarly for the Navy. This is understandable in the case of Weybrew, whose subject is, after all, submarines, but in other areas the significant literature is more evenly distributed. The subject index is so limited that it makes the book difficult to use as a reference work. For example, in this highly empirical field there is no entry in the index under "Measurement" or "Instrument." The call of the editors on page 454 for long-range planning is well-taken.

This book will be of use to the engineer or scientist who is already working in one aspect of the field of "bioastronautics," to give him a background in other subspecialities; it will also be of use to the physiologist or psychologist who is interested in entering the field, providing he is already versed in the language and mechanics of the physical aspects of space travel. For the engineer or physical scientist who wishes to review or enter the field of "bioastronautics," the language is rather too specialized.—*J. P. Nolan, Jr.*

Magnetism & the Chemical Bond by JOHN B. GOODENOUGH; 393 pages; $12.50; John Wiley & Sons, Interscience, 1963.

Extraordinarily well documented with some seven hundred references, most of them to very recent papers, this book is a careful, concise, and complete, if not lucid, account of the several diverse magnetic effects in solids containing transition metals, the structure of these solids, and the connections between magnetic effects and structure. The theoretical principles that govern are carefully developed, without oversimplification or prejudice, making use of the most recent advances in the theory of chemical bonding. Many tables of data are included, and many figures are given to illustrate the experimental facts. This book is important for anyone interested in magnetism or in the chemical bond, and it should be indispensable to research workers active in the science of materials.—*Robert G. Parr*

Diagnostic Methods in Speech Pathology by WENDELL JOHNSON *et al.;* 397 pages; $7.50; Harper & Row, 1963.

Diagnosis is the cornerstone of the therapeutic trilogy of diagnosis, prognosis and therapy. It should never be confused with the rhetorical problem of choosing names for disease syndromes. Symptom complexes group themselves into meaningful syndromes by virtue of common etiology or prognostic similarity. It is these common elements in

symptomology and prognosis that make therapy either reasonable or teachable. The age of idiosyncratic disorders and idiosyncratic therapy differing for every patient should have long ago gone the way of the phrenologies and pseudo-sciences and for the same reasons. The clinician's injunction to be responsive to the individual needs of each patient must never be at the expense of finding the common and non-individual aspects of the disease, if the clinician is to do more than merely ameliorate the suffering of patients he can treat in his own lifetime. The best clinician does more than treat, he learns and teaches and it is to that clinician that this book is addressed.

The book presents both a wealth of normative data and the diagnostic tools necessary for the comparison of symptomology across and within disorders of communication. The book is in some respects already an old friend of the practising speech pathologist, in that it is a revision of the authors' earlier *Diagnostic Manual in Speech Correction* published in 1952 by Harper and Brothers. Like many an unseen friend, however, this volume has changed considerably in its revision and fully justifies renewing the acquaintance. The authors, with the help of many unmentioned students and colleagues, review the literature relevant to the diagnosis of articulation problems, general language impairment, voice quality and stuttering. For each of these communicative disorders, specific diagnostic tests and procedures are given in considerable detail. In addition, standardization data culled from the literature are presented for both normal and dysfunctional speakers. These data alone, brought together in a convenient single source, would amply justify the procurement of the book.—*Murray S. Miron*

Alternating Current Polarography & Tensammetry by B. Breyer & H. H. Bauer; 288 pages; $12; John Wiley & Sons, Interscience, 1963. Vol. XIII of Chemical Analysis Series.

The past three or four years have witnessed an awakening appreciation of the power and sensitivity of alternating current voltammetric methods in the study of electrochemical reactions. Despite minor shortcomings it may display, and taken within the context of a rapidly evolving area of research, the appearance of this little book, which comprises the first monograph review of alternating current polarography and tensammetry, should afford a considerable measure of satisfaction to persons experienced in this field as well as to the growing number seeking to apply periodic relaxation methods to the study of electrode dynamics and to chemical analysis.

The discussions provide what amounts to a somewhat uncritical assay of important developments and revisions in the theory, instrumentation and applications of electrochemical A.C. current-voltage work done in the period from 1938 to 1960, and bring together for the first time in one place a diversity of work, some of which, from the early Australian school, had been reported in journals which remain largely inaccessible to many interested workers. The book is oriented almost exclusively to "sine wave polarography," but some less detailed discussion of the theory and practice of square wave polarography, faradaic rectification and the Fournier Effect is included for comparative purposes.

The three-year hiatus following the last year of literature coverage (1960) until actual publication of this book is indeed most unfortunate since this has resulted in, *inter alia*, only brief mention of harmonic studies and a complete absence of material describing newly-developed measuring circuits employing operational amplifiers, which, themselves, have come to represent almost the *sine qua non* for convenient and accurate measurements in this area of work; recently available commercial instruments of this type will doubtless lead to a rapid broadening of research interest in A.C. polarography.

This book by Breyer and Bauer is certainly to be recommended to anyone desiring an assessment of the principles of alternating current voltammetric methods in electrochemistry.—*Edward D. Moorhead*

Thermostatics & Thermodynamics by MYRON TRIBUS; 649 pages; $10.95; D. Van Nostrand Co., 1961.

The reviewer's task would be extremely easy if this book were to be evaluated as merely an attempt to place the discipline of "thermodynamics" before the student. However, the author has gone back for his primitive notions, not to classical statistical mechanics, but to the statistical methods of information theory in the belief that "students can, in shorter time, reach a state of understanding heretofore impossible for more than a small number of people." Only time and use can test this belief.

As a text, the book is well organized and very well written. The conversational tone adopted by the author surrounds the mathematical developments with an air of understanding that can be of prime importance to the student. A number of factors contribute to softening the mathematical terseness one usually expects—the use of word equations and analogies; summaries of concepts, mathematical methods and conclusions; and comments designed to put the subject matter in historical perspective. Another characteristic which will not fail to impress the student is the breadth of material from which both the exemplary and review problems are drawn. This material spans the range between casualty statistics of the Graeco-Persian wars and the operation of jet engines.

A connection between statistical mechanics and information theory cannot be established merely on the basis that the same mathematical expression occurs for "entropy" and "uncertainty" in the two respective fields. Due credit is given in the book to Professor E. T. Jaynes who recently pointed this out (*Phys. Rev.*, 1957) and, further, reinterpreted statistical mechanics as a form of statistical inference, thereby identifying "entropy" and "uncertainty" as the same *concept*. In doing this, Jaynes has skirted around the necessity for hypotheses such as ergodicity and the equal accessibility of phase space which must supplement the equations of motion of classical statistical mechanics. In fact, Jaynes' two original papers make excellent adjuncts to this book.

This formalism has not achieved universal acceptance: it is challenged chiefly by those who believe that the concept of probability which should be used as a foundation for physical theory must contain no element which is not, in principle, verifiable by the measurement of some frequency ratio or, at least, subject to a possible denial. Although this question is neither resolved nor resolvable at present, it detracts little from the quality of presentation in this book and, therefore, may not hinder Dean Tribus from accomplishing his objective.—*R. J. Mikovsky*

Uncertainty & Structure as Psychological Concepts by W. R. GARNER; 369 pages; $8.25; John Wiley & Sons, 1962.

In a sense, a scientific theory is a vision of the world and its value depends on how much it sees and how clearly. By this standard, the applications of information theory to psychology can be counted successful, and, in his book, Garner gathers the accomplishments and gives them a systematic presentation. The result demonstrates the beauties of dedicated and judicious scientific thought. Information theory had its short period of ascendency in which it was supposed to solve all the problems of psychology, then a sharp decline when many of its former adherents switched to deprecation. Through all this, Garner has remained steadily at his post, listening, experimenting, and performing the needed algebraic calculations. His book shows what has not been done, and what by its nature cannot be done.

The main thesis of Garner's book is that total amount of organization in a system (in the performance of a subject, for example,) can be measured as the total amount of predictable or interrelated variation in behavior; that is, in his terms, the amount of constrained uncertainty. Furthermore, constraint, if measured by standard uncertainty measures, can be partitioned by an

additive theory. Garner's main division is into internal and external constraint. Roughly speaking, internal constraint is self-consistency, and external constraint is adjustment to the outside world or task requirements. Following his earlier writings with McGill on the analogy of information theory with analysis of variance, Garner develops a theory of "main-effect" constraints and "interaction" constraints. The analysis is psychologically apt and displays more philosophical depth than is commonly found in psychological writing.

A major accomplishment of the book is the integration and coherent discussion of a multitude of studies of pattern perception, language, sequential behavior, and concept formation. There are 275 references, mostly to experimental results, and somehow they are woven into an intelligible texture of findings. What is more, Garner's interpretations of detailed results are far clearer and deeper, and often more detailed, than those accomplished by the authors of the experimental reports.

Interestingly, this is a case of a mathematical theory which does not predict much and is not really tested, yet clearly is useful as a way of grasping and arranging masses of data. Still, Garner is not limited by conventional information theory and often goes beyond its concepts in explaining behavior and laboratory results.

This is a well written, sound, organized, and integrated account of a mass of experimental and conceptual work. It is a must for advanced psychology students in all fields, and should be a cornerstone of any new psychophysics.—*Frank Restle*

History, Psychology & Science: Selected Papers by Edwin G. Boring, edited by Robert I. Watson & Donald T. Campbell; 372 pages; $8.95; John Wiley & Sons, 1963.

For Watson, historian of science, and Campbell, epistemologist and dialectician, the selection and editing of these delightful papers of Edwin Garriques Boring must have been a real pleasure. His keen intellect ranged widely and penetratingly over the psychological issues of the day. He had the rare ability to become deeply and personally involved in the most impersonal of issues, and the true scholar's feeling of personal responsibility for the growth and development of his discipline. From his chair at Harvard he presided over the destinies of some forty years of American psychology with rare wisdom, loving insight, and an occasional waspish delight in not sparing the rod when the incisive verbal disciplining of a wayward child seemed necessary.

For Boring, the publication of this collection is a deserved recognition of an unusual and distinguished literary talent. If William James wrote psychology like a novelist, Boring writes history and theory like an English essayist with a style in which a deceptive and occasionally almost chatty informality never masks the fundamental, careful elegance of his structure and expression.

For the reader, the result is both exciting and charming. The familiar topics are all here—the place of the great man in history, Zeitgeist and the psychology of science, the psychology of controversy, operationism in science, the mind-body problem, and some delightful bits from his long editorship of *Contemporary Psychology*. Their appeal is not limited to psychologists alone, nor to scientists alone for that matter. Any alert intellect will enjoy the exercise provided in these pages.

For the reviewer, the volume provides a chance to say "well done" to the author and the editors, and "congratulations" to the publishers for their good taste in both content and format.—*William A. Hunt*

Mechanisms Concerned with Conception, edited by Carl G. Hartman; 526 pages; $17; The Macmillan Co., Pergamon, 1963.

The proceedings of a symposium sponsored by the Population Council and the Planned Parenthood Federation, attended by more than 70 qualified students of fertility and reproduction, are herewith presented. The reports,

covering topics of Spermatogenesis, Physiology of the Male Accessory Organs, Composition and Physiology of Semen, Sperm Migration in the Female Genital Tract, Oogenesis and Ovulation, Fertilization and Transport of the Ovum, Implantation Mechanisms, and Immunological Phenomena, presented by research workers in the respective fields, form the basis of the text.

These data, first presented in 1959, have been updated to 1962. The contributors had the unusual opportunity of revising the text in the light of continuing work in this active field. A list of 150 problems that need investigation has been compiled in the course of the discussions and is in the appendix to the text. This interesting accumulation of unknowns is a challenge to students of conception.

The scope of knowledge of the mechanisms of fertility as well as control of conception is clearly defined. The experimental methods for the control of conception by such sophisticated means as ovulation inhibition, immunological reactions affecting sperm production and viability, factors favoring implantation, etc. constitute some of the described approaches to population control.

The presentations are very substantial and adequately illustrated. This book is highly recommended for the practicing physician and the research scientist.—*William Kleinberg*

Symbiotic Associations (Thirteenth Symposium of the Society for General Microbiology), edited by P. S. NUTMAN & B. MOSSE; 356 pages; $9.50; New York: Cambridge University Press, 1963.

Organisms are not always what they seem to be. Reading through the fifteen papers in this collection one is reminded that not only are lichens composed of separable and distinct groups of organisms but so are trees, bacteria, cows and orchids. In many biology courses it is probably not made clear that these obligate spatial associations are so widespread and general. From this symposium, it seems that there is no major group of animals, plants, or microorganisms which does not include several examples of symbiotic associations. Legumes and other plants fix atmospheric nitrogen in symbiotic association with bacteria, coral animals supply nitrogen and phosphorus wastes to vast intracellular populations of algae which, in turn, facilitate coral reef building by utilising carbon dioxide and maintaining the internal pH. Pine trees absorb nutrients from the soil more efficiently through infecting fungal hyphae than through their own roots, arthropods are "veritable cultures of microorganisms" and cows are aerobes surviving on otherwise non-utilizable foods by the maintenance of an internal anaerobic culture flask—the rumen.

These papers are concerned with the "origin, organization, and functioning of symbiotic systems," especially "ecology and taxonomy, methods of transmission, and metabolic interactions between symbionts, particularly at the nutritional level." The directives of the editors have been complied with by most of the contributors and the result is an exciting and probably important book. The emphasis of the different reviews varies, reflecting in an interesting way the preoccupations of the different groups of workers. Most of the papers have detailed discussion of the biological advantages accruing to the partners, with more or less incomplete descriptions of their biochemical and mechanistic bases. In contrast, a discussion of *mechanisms* comprises almost the whole of the article on phage lysogeny, this analysis being by far in advance of that in any of the other systems, while the biological basis of this "symbiosis" can hardly be guessed at.

This symposium, like most of its excellent predecessors, should be an obligatory part of the library of any Biology Department. The general view of biology given by it is novel and realistic and it suggests almost endless possibilities for ecological, genetic and biochemical investigation.—*W. D. Donachie*

Biochemistry & Physiology of Plant Immunity by B. A. Rubin & Ye. V. Artsikhovskaya; 358 pages; $14; The Macmillan Co., Pergamon, 1964.

Rubin and Artsikhovskaya are distinguished leaders of Russian research on the resistance of plants to disease.

It goes without saying that resistance overrides susceptibility to disease in plants as well as in animals. Were this not so, plants and animals would have disappeared from the earth. One hears these days much talk that plants and animals are chemically alike. Whatever be the chemical similarities, when an animal dies, it loses its resistance to microbial invasion; innumerable plants do not. Witness the cedar fence post, the chestnut rail, and the oak leaves.

Plant pathologists, however, have been unduly impressed with the passive, dead-tissue resistance of plants. Bereft of phagocytes and classical antibodies, living tissues of plants *must* contain anti-microbial chemicals like those that can be isolated from rot resistant logs, phenols, alkaloids, fungitoxic sulfur molecules, amines, and the like. Very few such compounds were ever implicated in resistance of living plants, however.

Within the last couple of decades, the emphasis has changed to considering that resistance in plants has a component associated with life—that resistance is a living, dynamic process that it is not necessarily due to poisonous chemicals lying in ambush for an invading pathogen.

Rubin and Artsikhovskaya have played an important role in this new look. Resistance is still chemical in nature but the chemicals are synthesized at the time of—and by the stimulus of the attack. In Rubin's Russia these compounds are called phytoncides—killers from living plants. Müller, formerly of Australia, calls the substances, phytoalexins, warding-off compounds in plants.

The theory behind phytoncides or phytoalexins is producing new knowledge about plant resistance in rapid order. This book summarizes very well indeed our present knowledge. It will provide a firm base. Those concerned with disease resistance in plants or animals will wish to have it.—*James G. Horsfall*

The Web of the Spider by L. B. Lougee; 44 pages; $3.50; Cranbrook Institute of Science, Bloomfield Hills, Michigan, 1964.

This book is a delight to the eye. The author has prepared exhibits of spider webs for the Cranbrook Institute of Science, and her art director-husband has designed the book and directed its production from the exhibits. There is much to be learned from just looking at the animals and their webs. The quality of the pictures seems to justify the fact that they take more space in the book than the text. It may be regarded as a drawback that the writing has been kept so brief that every sentence had to be loaded with information; but it is gratifying to notice that it is still clear and readable. This is not a scientific book and many observations which seem to the reviewer of interest in connection with spider webs are not discussed. The fact, however, that the pictures were prepared directly from photographs and the method of collecting and preserving spider webs is clearly described makes the book useful for the interested naturalist as well as for the more sophisticated researcher.—*Peter N. Witt*

Solar Flares by H. J. Smith & E. V. Smith; 322 pages; $12.95; The Macmillan Co., 1963.

The advent of radio astronomy, the IGY, and space research has moved solar flares from the realm of photographic solar patrols into a rapidly developing subject of concern to many overlapping areas of science. This book fills the resulting need for a coherent summary of existing observations. It is well written and organized, informative to the scientist or amateur astronomer desiring an introduction to solar flares, yet sufficiently comprehensive (especially in Chapters II and III) to be a useful handbook for workers in the field, with a good bibliography and index. Chapter I is a summary con-

cerning the structure of the solar atmosphere and the most important types of solar activity. The many (and frequently bewildering) specialized terms used in the literature are carefully delineated.

Half of the book is a survey of the photographic observations made in the last three decades, conveniently divided into monochromatic and spectral flare characteristics (Chapters II and III, respectively). Emphasis is placed on the severe limitations imposed by each type of observation and by the frequently low number of events in statistical investigations. Care is taken to present both sides of apparently contradictory observations and interpretations, but the authors do not hesitate to express their own opinions also. General results which are frequently quoted as facts in theoretical papers are shown to be far more ambiguous than theoreticians would like them to be.

Chapters IV through VI review in a less detailed manner the solar radio and corpuscular emissions, associated geophysical phenomena, and the main existing theoretical explanations of flares. Details which are soon to be outdated are minimized; emphasis is placed on a qualitative description of the many different types of observed phenomena. The extremely rudimentary state of theoretical explanations amply justifies the very descriptive nature of the whole book.—*Donat G. Wentzel*

Paleocurrents & Basin Analysis by PAUL EDWIN POTTER & F. J. PETTIJOHN; 296 pages; 30 halftone plates; $10; Springer-Verlag (Academic Press), 1963.

Many features of sedimentary rocks indicate the direction of ancient currents and, by extension, some of the characteristics of the basin in which these rocks formed. The vast extent of the literature on these subjects has created a need for the systematic treatment of the available data. This book answers this need in a very satisfactory way by integrating the nature, origin, and utility of the many sedimentary structures that indicate current directions. The authors have given us more than a scholarly review of the subject, including the historical background; they have added to it much of their own work, some of it previously unpublished, and their distinctive contribution of the unifying concept that ties it all together, what is called the "Basin Model."

Potter and Pettijohn have focused attention on ancient rocks and present forcefully the point of view that most sedimentary structures are better understood by careful observation of ancient rather than of modern sediments. In this spirit they have emphasized the usefulness of structures for the determination of paleocurrents and have given relatively little consideration to fundamental fluid mechanics by which the structures are produced. In writing this book, then, they have had the approach of the historical geologist rather than that of the student of any particular set of sedimentary processes.

This attitude dictates the chapters on "Dispersal and Current Systems" and "Basin Analysis and the Sedimentary Model" being the summation of the book. I know of no other place in the geologic literature where these subjects have been tackled with such competence and salutary results.

The book will have many uses: for recognition of the many sedimentary structures, in particular the sole markings; as an introduction to the methodology of study of those structures; for pointing out the relationships between the results obtained by the stratigrapher, the petrologist, the subsurface geologist, and the geophysicist. Not the least of its virtues are the extensive list of references at the end of each chapter and the useful indices (subject, author, formation).

The high mark of the book is the collection of the 30 plates, superb photographs that are the next best thing to seeing these features in the field. The same authors' atlas and glossary of sedimentary structures, which will be forthcoming soon, will no doubt be an even more impressive visual display. I hope that, in that book, they will convince their publishers to use larger type for illustration captions.

I cannot see that a sedimentologist or stratigrapher will have a complete shelf of books without this publication.—*Raymond Siever*

Physics & Chemistry of the Organic Solid State, Vol. 1, edited by D. Fox *et al.;* 823 pages; $25; John Wiley & Sons, Interscience, 1963.

Interest in the properties of organic solids has been noticeably increasing during the past two decades. Contrasted with the explosive rise of knowledge about inorganic solids such as metals, semiconductors, and ionic crystals, and the use of the electronic properties of these materials, the organic field seems to have been relatively neglected. A large part of this result is certainly due to the fact that physicists and physical chemists found it easier to apply their knowledge to inorganic crystals than to organic ones; it is not because organic crystals have less remarkable properties.

The book under review should provide a relatively easy and broad introduction for physicists and chemists to the current state of knowledge of the organic solid state. It contains the most complete review of the thermodynamic properties of organic solids now in print, accompanied by an extensive review of the literature of this subject. This is supplemented by a chapter on "plastic crystals," i.e., those which have molecular motion in the solid and therefore display unusual thermodynamic, mechanical, and electronic properties.

A chapter on photochemical reactions gives a brief account of the systematics which have been discerned in this area as well as some of the bizarre reversible and irreversible processes which can occur. Probably the unique photochemical behavior of organic crystals, more than any other property, distinguishes them from inorganic ones. Four chapters dealing with electronic properties—electronic absorption, infrared absorption, dielectric phenomena, and electron transfer—illustrate the special behavior of molecular crystals. A significant omission from the book in this category is the rapidly developing field of exciton motion.

Several other chapters—crystal form and structure, structure of surfaces, crystallization of polymers, thermal reactions of solids—help to complete the picture and provide food for thought and imagination. The excellent chapters on purification and crystal growth should encourage definite steps toward the realization of new ideas.—*Donald S. McClure*

Brains, Machines, & Mathematics by M. A. Arbib; 152 pages; $6.95; McGraw-Hill Book Co., 1964.

In less than 150 pages Michael A. Arbib romps through the thickets of neural nets, finite automata, Turing machines, the relation of structure to randomness, the reliability of brains and similar devices, the crucial notions of cybernetics, with an appendix on set theory, linking it to modern secondary school education.

The author gives his readers a sparkling primer of one of the fastest growing fields of science. It is written for the intelligent and interested ignoramus, as each of us is in another's science. It follows the development of all of the crucial ideas, conveying their substance to the uninitiated, and displays them in their logical relations.

The style is fluent and informal, but the definitions are crisp and the argument sharp. Perhaps its most remarkable achievement is that even a high school student can set the book down and come away with a knowledge of Gödel's theorem, a realization of its relevance, and an understanding of its proof.

The notes from which it sprang were the substance of a dozen popular lectures recorded and still in vogue in adult education in Australia. The text will be used as the basis on which to build courses in which biology and technology are married to fit students for the challenge of our age. They will enjoy the brevity of its wit and be grateful for the critical hints of other things worth reading.—*W. S. McCulloch*

Dynamics and Thermodynamics of Planetary Entry by W. H. T. Loh; 268 pages; $12; Prentice-Hall, Inc., 1963.

In the preface to this book on entry dynamics and thermodynamics, the author states that his purpose is to make available to students, engineers, and scientists "those recent works on re-entry and planetary entry which would be suitable as an introductory text in the subject as well as a reference work for more advanced research." In a certain sense, Dr. Loh has achieved his goal for he has indeed made available in book form a collection of the results of a number of the outstanding contributors to the field of entry physics and dynamics. This reviewer, however, must confess that he was somewhat disappointed by the over-all impression made upon him by Dr. Loh's book. A great deal of the material presented in this volume is set forth with such brevity that the text takes on the appearance of a set of edited lecture notes. This is most unfortunate, for the style chosen has tended to keep careful discussion and critique of many of the basic assumptions made and the physics underlying these assumptions to a minimum. The ultimate result of this choice of style is that Dr. Loh's book is more a compendium of existing technique than it is a definitive treatise on entry dynamics and thermodynamics.

The book is divided into three parts. The first, and by far the largest, part of the book is concerned with the point-mass dynamics of entry vehicles. The author has performed a valuable service here in drawing together the diverse work of a number of original contributors to this field and exhibiting the relationship and interrelationship of the many approximate point-mass entry solutions which exist in the literature.

The second part of Dr. Loh's book is concerned with the aerodynamic heating of re-entry vehicles and the behavior of ablative heat shields. In this portion of the book the author presents a very brief and "broad-brush" look at the problems of thermal protection of entry vehicles. The presentation is limited to the very general methods that are in every-day use by systems engineers who are primarily interested in the specification and gross performance of heat protection systems.

The final part of this book is very

short and is devoted to two rather disjointed topics—the dynamics of spinning ballistic missiles and an introduction to the basic equations for orbital mechanics. Both topics are treated in very brief outline form and it is a little difficult for this reviewer to understand the logic behind the inclusion of this material in the present volume. It would have seemed more appropriate to have included in the final part of this book a discussion of the short period motions of entry vehicles and their general effects on trajectory dynamics. Indeed, some of the analytical work of the staff of the Ames Laboratory of NASA on the dispersion of entry vehicles due to angle of attack misalignment is so closely related to the discussion in the first portion of Dr. Loh's volume that it is difficult to see how the author could have resisted the temptation to include this material in his book.

In summary, then, the author of this short book on entry into planetary atmospheres has provided an exposition of the analytical tools that are presently available to the systems engineer for determining the point-mass dynamics and general heat shield requirements of a broad spectrum of entry vehicles. The volume should prove useful to the many engineers who, at the present time, are engaged in such studies.—*C. Donaldson*

Dynamics of Manned Lifting Planetary Entry, edited by S. M. SCALA, *et al.*; 980 pages; $15; John Wiley & Sons, Inc., 1963.

This volume presents the proceedings of the third in a series of symposia cosponsored by the Air Force Office of Scientific Research and the General Electric Company on the fundamental aspects of the aerospace disciplines. It has become the custom in recent years to publish the proceedings of conferences and symposia such as these in book form. In spite of some of the shortcomings of this method of publication—the most often heard objections being slowness of publication, non-uniformity of quality, and lack of continuity among the papers included in the proceedings—the present reviewer is, on the whole, in favor of this method of publication.

Happily, in the case of the present volume, the general level of quality of the papers presented and the excellent job of achieving uniformity of style that has been accomplished by the editors only tend to strengthen the opinion of this reviewer that this method of presenting the transactions of symposia is highly desirable.

There are thirty-four technical papers included in this volume. These papers are presented according to the symposium plan under one of the following six separate sections: 1. Planetary Atmosphere Models. 2. Aerodynamic Plasmas. 3. Energy Management. 4. Aerothermoelasticity. 5. Low Speed Aerodynamics. 6. Hypervelocity Flows.

It would not seem appropriate to review at this time each of the thirty-four papers which comprise this volume nor does it seem right that only a few of the more outstanding papers be selected for review. Instead it would seem proper to comment on the over-all impression given by this volume and to point out some of the general areas in which it is felt that new and useful information is presented.

First of all, the general quality of the papers presented at this symposium was high, and, in spite of the fact that many of these papers tend to present a rather broad picture of the state of the art, their technical content is such that they are not intended for the consumption of the non-professional. These proceedings will, therefore, be found useful and enlightening by the serious professional engineer or scientist. Although the whole spectrum of entry technology is covered in this volume, some areas are covered in somewhat more detail than others. Those areas which have been given somewhat more coverage than others should be mentioned. They are communications in plasma media, chemical kinetics in high temperature gases, and the aerodynamics and handling qualities of manned entry vehicles which are capable of tangential landing capability.

The editors and publishers of this book on entry technology should be congratulated for having turned out a very creditable volume reporting the proceedings of the third AFOSR-GE sponsored symposium on the aerospace disciplines.—*C. Donaldson*

International Auroral Atlas by J. PATON *et. al.*; $8.75; Aldine Publishing Co., 1963.

The *Atlas* is an outcome of the second international effort to improve the classification of the aurora which is one of the most complicated, but yet one of the most fascinating phenomena in nature. The difficult task was accomplished by a subcommittee of the International Association of Geomagnetism and Aeronomy (IAGA), with the chairman J. Paton.

The *Atlas* certainly reflects the advance in our knowledge of the aurora after the first edition was published in 1930, under the leadership of the late Carl Störmer. Instead of simply grouping various types of the auroras, a more systematic (or even ambitious) classification is attempted in the new *Atlas;* auroral characteristics are described in terms of *condition* (*cf.* quiet (q), active (a)), *structure* (*cf.* homogeneous (H), rayed (R)), *form* (*cf.* band (B), patch (P)), *brightness index* (0, 1, . . . , 4), and *color class* (a, b, . . . , f). For example, "aRB3b" signifies an active rayed band with the brightness of order 10^2 kilorayleighs and with a red lower border.

Based on the new classification, various auroral forms are then illustrated by (37) photographs, both in black and white (32) and color (5). Most of the black and white photographs are well chosen and will serve, as typical examples, for the purpose. They are further supplemented by 20 all-sky camera photographs. The color photographs are, however, far from reality.

It is fortunate that the *Atlas* is now available to those who participate in the International Year of the Quiet Sun (IQSY) which is now underway. The practical usefulness of the new classification will certainly be tested by them.—*S. I. Akasofu*

The Physiology of Insect Senses by V. G. Dethier; 266 pages; $7.25; John Wiley & Sons, 1963.

Insects differ so much in external form and habit from ourselves that it is perhaps surprising to find their basic functions to be often very similar to ours. This is nowhere truer than in their sensory physiology, that is, in the manner in which changes in the external world are translated by insect sense organs into patterns of nerve impulses. Dr. Dethier shows that the difference is mainly one of complexity, the small size of insects requiring that they accomplish with a few sense cells tasks of detection for which we employ hundreds of times as many detector units. The physiologist can put this characteristic to good use, for it is much easier to relate the operation of such a simple system to how the whole animal responds. For instance, Dr. Dethier shows that a microscopic drop of sugar placed on a single receptor cell on a fly's proboscis is capable of releasing the full pattern of feeding behavior. No cause-and-effect relation could have a simpler starting point—at least at the cellular level. The book provides a thoughtful and balanced overview of recent work on the major insect senses, documented with forty-one pages of references. Full attention is given to the structural organization of receptors as well as the physiological and behavioral evidence of their function. If the book has a weakness it is a lack of logical underpinning to some of the electrophysiological statements. Otherwise, this is an eminently readable account that somehow manages to span levels of interest from college freshman to postdoctoral fellow.—*Kenneth D. Roeder*

Radio Ray Propagation in the Ionosphere by J. M. Kelso; 408 pages; $17.50; McGraw Hill Book Co., 1964.

This book deals with the subject of geometrical optics of radio waves in the ionosphere. The theoretical and experimental aspects are covered about equally. The author himself has made substantial contributions to the subject and these portions of the material are

particularly well done.

Following an interesting historical introduction, the author gives a lucid description of the theory of the propagation of electromagnetic waves in a homogeneous isotropic media. The sections on elliptical polarization include some valuable charts based on the author's original work.

The next chapter is a readable survey of the physics of the ionosphere. This sets the stage for the following chapter which deals with the dispersion equation for propagation in the ionosphere including the effect of the earth's magnetic field. The final two chapters deal with ray theory and procedures for ray tracing in the ionosphere.

In the opinion of this reviewer, the basic shortcoming of the book is the omission of an adequate discussion on the validity of ray theory and the relationships with wave theory. Full wave methods, which are promised for a second volume, should be part of the diet of current ionospheric research workers. With the availability of digital computers, many of the ray approaches to the subject are giving away to a direct frontal assault on the coupled differential equations which describe the field components in the medium. Nevertheless, the ray techniques, which are so well covered by Kelso in this book, will be with us for some time.

For a first-year graduate course of lectures on radio propagation, the book would be indispensable. Also, apart from its obvious value to ionospheric research workers, the book should be a valuable reference for plasma physicists.

The excessive price for such a slim volume is rather appalling but not too surprising in these days.—*James R. Wait*

Radioactive Dating (Proceedings of Symposium, Athens, November 1962); 440 pages; $8.50 paper; Obtainable from International Publications, Inc. & UNESCO Publications Center, 317 E. 34th St., New York 10016.

The stated aim of this symposium was to "reflect the present status of radioactivites dating techniques as they are applied in the various disciplines." This is a very ambitious statement which unfortunately can be only partially fulfilled in the field of geochronology.

The thirty-one papers by nearly sixty authors are grouped into four sections: new methods and possibilities; geochemistry and geophysics; geology; and meteorites. Naturally, these divisions are somewhat arbitrary since a number of the papers could reasonably fit into more than one cateogory.

Technically the book is quite good with clear type and a minimum number of typographical errors. Many of the papers contain very valuable information not easily obtainable elsewhere. However it shares, with other symposia volumes, several inherent weaknesses. First, the rate-determining step in such a volume's publication is the preparation of the most tardy manuscript—apparently about one year in this case. Second, in such a collection, coverage of any one subject is likely to be rather incomplete as not all workers within a field can attend such a symposium. Most significant, however, is that publication of a bound volume imparts an aura of inviolability and finality to the papers included within it. Thus, if the refereeing system for the symposium is not as stringent as in most journals, there is heightened risk of including reports of less than the best quality. One of the more recent developments in geochronology, the fission track technique of Fleischer, Price, and coworkers, could not even be mentioned in the text because its publication postdated the symposium.

I can recommend this book for inclusion in Institution libraries or for the shelves of those specialists who may be involved in research in one of the fields covered. I do not believe, however, that this book can be used either as a survey of or an introduction to the subject of radioactive dating. The coverage is both too detailed and uneven to allow the unfamiliar reader to gain an over-all impression of all of the recent developments in geochronology.—*Michael E. Lipschutz*

Handbook of Physiology, Section 4: Adaptation to the Environment, edited by D. B. DILL *et al.;* 1056 pages; $32; American Physiological Society (The Williams & Wilkins Co.), 1964.

This monograph on Adaptation to Environment represents an important contribution to biological literature. It is not an encylopedia nor a compendium, but rather a series of chapters on various aspects of adaptation written by experts in these particular subjects. The word "Adaptation" is used to encompass nearly every aspect of an organism's response to or relationship with its environment. This book is written from the physiologist's outlook on nature with especial emphasis on the effects of the physical environment. The editorial work is excellent.

The first three chapters are introductory to the monograph and give philosophical as well as physiological perspective. The fourth chapter, written by an eminent Russian physiologist, on the subject of cellular adaptations gives an excellent basis for the next ten chapters. These chapters concern adaptations in the various organ systems to environmental stresses. Each chapter is written by an expert in the field. The chapter on respiration is particularly well done.

A great deal of emphasis has been put on adaptations to desert and environmental temperatures, and cold arctic temperatures. Temperature regulation is also discussed. Six chapters by various experts comprise this portion of the monograph. These chapters are very well illustrated and leave the reader with a fairly clear mental picture of where and how much of the research has been accomplished in these various fields.

Authorship is international in character. There were 66 authors involved and each wrote lucidly and concisely. There are some gaps. Adaptations of animals (exclusive of man) to high altitudes is not treated as well as one would wish. Adaptative radiation of animals is not discussed.

Two chapters on adaptation of man to high altitude, one by a Peruvian physiologist and the other by a British physiologist and mountaineer are par-

ticularly outstanding. They seem like a grand climax to this excellent monograph.

The bibliographies are adequate and will be very useful. The (subject) index is also very well compiled. The editors have done well in welding all of this material into one noteworthy contribution. This book should be in every biological library.—*F. G. Hall*

Biochemical Engineering, An Introduction by F. C. Webb; 743 pages; $18.50; D. Van Nostrand Co., 1964.

Biochemical engineering may be defined as embracing those aspects of chemical engineering, biochemistry, and biology which are important in the design and operation of industrial units using biological processes or raw materials. The scope of the book reflects the breadth of this definition. The first four chapters provide an introduction and discussion of microbiology and biochemistry. These are followed by five chapters dealing with colloids, emulsions, reaction kinetics, thermodynamics, and oxidation-reduction potentials. Next come chapters concerned with mass transfer, heat transfer, air compression and sterilization, chemical disinfection, and preservation of biological materials by cooling, drying, vacuum drying, and irradiation. Product isolation and equipment design are covered in two chapters while the remaining ones deal with production of enzymes, yeast, antibiotics, other fermentation products, vaccines, and sewage treatment. At the end of each chapter are up-to-date references to reviews, symposia, and research papers of significance, which are quite useful. Unfortunately, many errors are noticeable in the references.

In comparison to an older book bearing the same title (Biochemical Engineering, R. Steel, Heywood, 1958) this book is somewhat superficial although much broader in coverage, not limiting itself to fermentation technology. In almost no instance is the development of a subject carried far enough to enable a student actually to design the process under discussion. Furthermore, although Dr. Webb of University College, London, has written this book as a result of his one year graduate course in biochemical engineering which is open either to biochemists or chemical engineers, it would be, in large part, redundant and too elementary for the engineer. The biologist, though, would receive an adequate introduction to the quantitative side of the food and fermentation industries.

The reviewer believes this book would be useful in an undergraduate survey course on biochemical engineering, but on the graduate level it would serve only to make unnecessary the first few minutes of each hour's lecture. For industrial readers interested in a small number of processes in a single industry, the book is clearly of too general a nature.—*Richard I. Mateles*

Entropy by J. D. Fast; 313 pages: $10.75; McGraw-Hill Book Co., 1962.

This interesting volume might more descriptively be titled "Selected Topics in Statistical Thermodynamics." The

actual title is a good one, however, for it is , among other things, a pedagogical device. The book " . . . endeavor(s) to reach all of those students and researchers to whom thermodynamics and statistical mechanics seem a little frightening . . . ," and for this purpose undertakes, so to speak, to attack the dragon head-on. This approach has some merit although, as taken here, it sacrifices certain elements of balance.

The treatment tacitly assumes the First Law of Thermodynamics (no mention is made of it in the index) but gives extended discussions of the Second and Third Laws, with special emphasis on their statistical interpretation. A good brief statement of quantum ideas makes possible a straightforward introduction of Bose-Einstein and Fermi-Dirac statistics, and the justification of Boltzmann statistics as the limiting form which they take in statistically dilute systems. Brief discussions are given of chemical equilibrium (for metallurgists), paramagnetism and low temperatures, interstitial atoms in body-centered cubic metals, substitutional alloys, ferromagnetism, vacancies and diffusion in solids, elasticity of rubber, solutions of polymers, radiation of heat and light, and fuel cells and heat pumps. Full chapters are devoted to the entropy of monatomic gases and to the entropy of diatomic gases. No mention is made of liquids, and the solid state is discussed on the level of the Einstein approximation, Debye being barely mentioned.

Being essentially a personal work, this book is open to personal criticism in such matters as choice of materials for inclusion, which explanations should be detailed and which sketchy, where emphasis should be placed, etc. It does a considerable number of things very well, however, and can be recommended either for "brush-up" study by those

who have left school or as collateral reading in a statistical thermodynamics course.—*Henry S. Frank*

Poisonous Plants of the United States & Canada by J. M. Kingsbury; 626 pages, 129 figures, 4 color plates; $13; Prentice-Hall, 1964.

This magnificent compilation should have been titled "A Veterinarian's Handbook to the Poisonous Plants of the United States and Canada." In any other sense it will scare the human being to a meal of dandelion greens and sugar cane, about the only two items of the human diet not mentioned as toxic. According to the author's lists a typical poisonous menu could consist of: appetizer—tomato; salad—avacado, lettuce, cucumber, and radishes; entree—mussels and fish made poisonous with dinoflagellates, and hallucinogenic with mushrooms; vegetables—beans, asparagus, corn, carrots, or mustard greens; desserts—apple or peach or rhubarb—followed by poisonous peanuts and, of course, *Nicotiana tabacum* completes the misplaced emphasis.

For the veterinarian the data on toxicity, symptoms, lesions, and poisonous principles are complete. The botanical descriptions offered are woefully inadequate and in some cases inaccurate. The figures may help but the photographs often seem to be those "on hand."

For the botanist who has worked with a poison center this book is invaluable as a reference to those plants reputed to have a toxic principle. Here in compact form is the summation of a bibliography of 1800 titles arranged partly according to subject and partly in a systematic manner according to plant family, genus, and species. Regrettably, the systematic arrangement used is not widely accepted and most botanists must resort to the index to locate the family in question. Frequently, synonymy is offered when various references have used different but equivalent names. The bibliography of historical interest from Dioscorides to the present follows the first chapter which originally was published separately. The remaining chapters have a combined bibliography which, on quick examination, ranges from 1887 to 1963, being both disconcertingly ancient and comfortingly up to date.

Modern problems are handled acutely. On the subject of "tobacco of commerce" the author states: "Poisoning in human beings and livestock is not infrequent from intentional or accidental misuse of nicotine or products containing it. The alkaloid is readily absorbed after either ingestion or inhalation or through scarified or intact skin and is rapidly fatal in small amounts. The discussion presented here, however, is confined to poisoning brought about by accidental ingestion of the plant." To the hardy reader who believes the current advertising that *Lobelia inflata* or its derivatives may aid in breaking the tobacco habit the author offers these comforting suggestions: "Overdose of the plant or extracts of the leaves or fruits produce vomition, sweating, pain, paralysis, depressed temperature, rapid but feeble pulse, collapse, coma, and death in the human being."

The most potent quotation of the book, however, is lost on page 306. The author states "Some of the present confusion concerning the toxicity of species of *Astragalus* and *Oxytropis* can be eliminated if published reports dealing with any aspect of their toxicity are accompanied by reference to the botanist making the determination of the experimental or field material and the deposition of labeled representative specimens in an accessible herbarium." Remove the four words "of *Astragalus* and *Oxytropis*" and this sentence belongs in a box on the front cover.—*Richard A. Howard*

Physics of Thin Films, Advances in Research & Development, Vol. I, edited by G. Hass; 350 pages; $13; Academic Press, 1963.

This book is the first volume of a new series of the "Recent Advances" type. During the past ten years bound annual volumes of this kind have been taking over the function previously served by

review journals. There is little doubt that such volumes of specialized reviews serve a useful purpose, even though they are primarily designed for scientific libraries and not for the individual scientist.

What does Volume 1 contain? There is first a 67-page article on ultrahigh vacuum evaporators and residual gas analysis. Anyone not familiar with ultrahigh vacuum equipment would hardly learn the necessary practical details from this article, which seems to be addressed neither to the specialist nor to the beginner. For example, we are told that "liquid nitrogen cooled traps have been used for many years in vacuum systems. Their design is not critical due to the high heat of vaporization of relatively inexpensive liquid nitrogen." The entire article is a collection of such bits and pieces of information and misinformation. It is neither a comprehensive review of recent work nor a critical introduction to the field.

The next article is on the theory and calculations of optical thin films. Although much of this material replicates the standard textbook of Born and Wolf, many practical details on calculations are given, and this particular 50 pages is good value for anyone faced with the necessity of making such computations. A 60-page article on reflecting coatings for the vacuum ultraviolet is a good technical review of a specialized subject, with reliable information conveniently summarized. The structure of thin films is discussed in 40 odd pages, an elementary and sketchy treatment of the subject, which devotes much attention to reproduction of commercial pictures of electron microscopes and textbook material. This article is worthless to the specialist in thin films, since it does not provide a comprehensive survey of the recent literature in depth. It may be a pleasant introduction to the subject for a student who knows nothing about it. A 40-page article on low temperature films is mainly devoted to a treatment of the theory of superconductivity. The book concludes with a 50-page article on magnetic films of nickel and iron, a good blend of scientific results and technical applications.

It may be noted that, of the six contributors to Volume 1, four come from IBM and two from the U.S. Army Engineers, as does the editor. The series will serve the useful purpose which is intended only if the editor will take care to exclude the sketchy and qualitative type of article which does not provide a complete and critical review of the literature. The reader turning to a review has a right to expect that it has been done carefully and completely and that he will not need to repeat the literature search which is reported by the reviewer. In fairness to the libraries which must buy these volumes, they should not be padded out with repetition of textbook material, illustrations from commerical handouts, and the like. Definite improvements in this series are necessary before we can accord it a place in our library subscription list. The same publisher recently started a series, "Progress in Nucleic Acid Research," which suffers from none of the defects noted in this review. Thus we are not setting up an impractical standard.—*Walter J. Moore*

Divalent Carbon by JACK HINE; 206 pages; $7; The Ronald Press Co., 1964.

Proclamation of upheaval and chaos in science is the fashion and lament of the time. Jack Hine might have written "The Crisis in Carbene Chemistry" or "The New Carbene Chemistry." Fortunately he did not. Instead he has ordered a burgeoning field, convincing his readers of the interest and significance of his subject without resorting to gambits of the scientific gamesman. Fifteen years after organic chemists were forced by Hine to recognize these strange beasts, "Divalent Carbon" stands as a milestone along a scientific superhighway.

This book offers a lucid and concise survey of the modes of formation and reaction of carbenes. It may be recommended as an excellent introduction to a field whose synthetic and theoretical fertility has been as spectacular as it has been recent.

A lesson in tight mechanistic reasoning is provided by Hine's discussion of reaction pathways. It is fitting that the man who proved the existence of divalent carbon intermediates by reaction kinetic studies should point to the need for care and a healthy skepticism in sorting the proved from the possible. Since carbenes are exceedingly reactive and consequently short-lived, the tendency to base mechanistic arguments primarily on the nature of reaction products can be understood—understood but not encouraged. That chemists' hands are *not* tied by the brief lifetimes of methylenes is amply demonstrated by Hine's exploration of the available mechanistic data.

No review is complete without a minor quibble. It is now known that the removal of certain metal mirrors is *not* a characteristic reaction of methylene. More regrettable because of its utter plausibility is the statement that there is compelling evidence for the insertion of methylene into a carbon-hydrogen bond by hydrogen abstraction followed by radical combination.

"Divalent Carbon" is an important book. For the casual reader seeking to acquaint himself with carbene chemistry it is an introduction, while for the organic chemist it is an invitation and for the dedicated partisan a reminder that this is a time for extending our knowledge. Jack Hine clearly indicates the guidelines and criteria for our progress. There is no summary chapter, for the rapid proliferation of carbene chemistry would have rendered it as quaint and premature as a tuxedo on a five-year old.—*Peter Gaspar*

The Mathematical Works of J. H. C. Whitehead, edited by I. M. JAMES; 1594 pages in 4 volumes; $45 the set; The Macmillan Co., Pergamon, 1963.

The research career, 1929–1960, of the late J. H. C. Whitehead, 1904–1960, coincided with a period of spectacular development of both geometric and algebraic topology. To that ever accelerating development, he and his school made contributions of the first magnitude.

Volume 1 of this complete collection represents an early interest (1929–1937)

in geometry, especially differential, and contains also some algebraic papers.

The last three volumes, topological in nature, correspond to three phases of Whitehead's research: (1) A geometric topological period, with emphasis on the classification problem for combinatorial manifolds, including the Poincaré conjecture. The latter, in dimension 3, surmises that the only simply connected compact 3-manifold is a topological 3-sphere. (2) An algebraic topological period, about 1947 to 1955, centering about homotopy theory and including some work on fiber spaces. (3) A final period, from 1957 until his untimely death, in which he returned to relatively geometric topology. He was stimulated to this return partly by the spectacular advances of some young topologists who found in Whitehead's earlier work many concepts, techniques, and results tailored to their needs. It was only shortly after Whitehead's death that a young mathematician, using an attack suggested by him, proved the Poincaré conjecture in higher dimensions. It remains unproved only for dimensions 3 and 4.

Particularly noteworthy are Whitehead's introduction and investigation of homotopy types, his treatment of homotopy theory in terms of CW-complexes, and his contributions to the study of fiber spaces.

Besides being a creative mathematician, Whitehead was a leader of research, regarded with affectionate admiration by the younger men whom he directly influenced. His many joint papers testify to a close personal and professional relation with numerous colleagues.

These collected works will facilitate the further extension of Whitehead's mathematical influence and will be an invaluable source for students and research workers in topology.—*Stewart S. Cairns*

Evolutionary Trends in Foraminifera, edited by G. H. R. Von Koenigswald *et al.;* 355 pages; $16.50; American Elsevier Publishing Co., 1962.

The appearance of this book—a collection of papers dedicated to I. M. van

der Vlerk on the occasion of his 70th birthday—is in the tradition of other scientific books issued by the Elsevier Publishing Co.: high quality of scholarship, high publishing standards (paper, print, and binding), and high price.

The articles gathered here are intended primarily to present an over-all view of the current status of research in various groups of Foraminifera, not to present new and original material. In this the authors have succeeded very well.

The collection includes fourteen articles of which six are in English, four in French, and four in German. The reviewer found the most stimulating contributions to be those of MacGillavry (Phylomorphogenesis and evolutionary trends of Cretaceous orbitoidal Foraminifera); Smout (The Genus *Pseudomia* and its phyletic relationships, with remarks on *Orbitolites* and other complex Foraminifera) and Drooger, (Evolutionary trends of the Miogypsinidae).

An unfortunate omission from this reviewer's somewhat prejudiced point of view was an article on the taxonomy and phylogeny of the Tertiary planktonic Foraminifera; it would have been a fitting complement to the article by Prof. Cita-Sironi.

It is interesting to note that several authors postulate a polyphyletic origin for the groups with which they are concerned, whereas others are equally steadfast in support of monophyly. This variation in opinion stands in marked contrast to our Soviet colleagues, where monophyletic origin and development is a cardinal rule in the organic world.—*W. A. Berggren*

Calculus & Its Applications by P. MAINARDI & H. BARKAN; 537 pages; $7.50; The Macmillan Co., Pergamon, 1963.

Depending on one's point of view, he will probably either strongly like or strongly dislike this book. In essence it is an introduction to calculus of outstanding clarity, but based on intuition rather than rigor.

The authors' approach is well illustrated by their treatment of the limit concept. They speculate on primitive man's gradual recognition of various instantaneous speeds as characteristic of different animals and outline the contributions of early mathematicians before presenting Cauchy's limit definition. Observing that the student may at first think this is "mathematical gibberish," they carefully illustrate by geometrical analogy. Similarly in the rest of the book, important theorems are stated and vividly discussed to show their plausibility, but not proved unless the proof would aid intuitive understanding.

The usual introductory topics are covered, including differentiation, integration, series, and analytic geometry. There are very brief treatments of differential equations, partial differentiation, and directional derivatives. Ample problems are provided, half with answers. Like the exposition, the problems have strong physical orientation. Offset reproduction of typed copy is used, but the equations are so well typed and the notation so simple that this is not a serious detraction.

Students desiring an understanding of introductory calculus from the standpoint of physical applications will find the book excellent for self-study. Teachers sympathetic to this desire will find it refreshing.—*Peter L. Balise*

Introduction to Vector & Tensor Analysis by R. C. WREDE; 418 pages; $9.75; John Wiley & Sons, 1963.

Although this text is somewhat similar to several other recent ones, it deserves notice both because of its distinguishing features and because of the expanding interest in vector and tensor analysis in science and engineering. Cartesian vector analysis is now commonly taught in elementary mechanics courses, despite somewhat justified objections that the manipulations may obscure the physical relations. And the more general concept of vectors as ordered n-tuples is increasingly taught in courses ranging from electric circuits to economics.

In accord with modern treatments, the **i**, **j**, **k** notation is never used in this book, the superscript notation being ap-

propriately introduced at the beginning, followed soon by the summation convention. Thus, as throughout the work, it is assumed that the reader has some maturity, perhaps that of a science junior or senior. As another example of the author's viewpoint, divergence and curl are defined without fluid flow interpretations. However, physical problems are quite frequently discussed.

The general approach is a presentation of the algebraic characteristics of vectors and the relations between algebraic and geometric expressions, emphasizing transformation theory. This is naturally continued into differentiation and integration of vectors. The last quarter of the book is devoted to tensor analysis, but some tensor notation has been used previously. The student's interest is enhanced by applications such as an introduction to special relativity, historical notes, and airbrush illustrations. The index is ample.

The book seems a good compromise between rigor devoid of physical illustrations, and physical presentations lacking in mathematical rigor.—*Peter L. Balise*

Introduction to Analog Computation by J. R. Ashley; 294 pages; $8.75; John Wiley & Sons, 1963.

Usually the hardest part of learning analog computation is getting started. After becoming familiar with the basic techniques, one soon develops ability to devise rather complicated programs and to search the literature for appropriate circuits. A good introductory book like this makes it easy to get started if a computer is available.

Appropriately, the presentation is based on interesting examples. The author keeps in mind the student's position, using familiar problems and developing the material logically rather than trying to impress the reader with erudition. The computer techniques are sound and in accord with modern practice. In particular, time scaling is shown to require essentially just simple changes in time-dependent elements. Amplitude scaling is handled by writing, for example, $x \equiv (x_{max}/C_{max})\bar{x}$, where

x_{max} is maximum expected value of the variable, C_{max} is maximum computer voltage, and $\bar{x}$ is voltage representing x. This reviewer prefers almost the same method, but using "computer units" instead of volts.

The book is only an introduction, without tabulated reference data. More advanced problems like partial differential equations are not mentioned. There is practically nothing on internal circuitry; even relay control circuits are only partly described. A digital computer example is given in the first chapter, but hybrid equipment is not discussed.

However, all the really necessary basic information is clearly explained, including nonlinear problems, simulation, and an appropriate selection of special circuits. The book is probably one of the two or three best introductions to analog computation.—*Peter L. Balise*

Administration of The Chemical Enterprise, edited by CONRAD BERENSON; 414 pages; $11.50; John Wiley & Sons, Interscience, 1963.

"Administration of the Chemical Enterprise" is a collection of nine chapters on matters of concern to managers, investors, employees, and observers of the Chemical Process Industry. The editor, Dr. Berenson, contributed three chapters covering Marketing, Management, and Human Resources. Others discussed were Mergers, Advertising, Patents, Sources of Business Information, Accounting, and Financial Management. Production and Research are not considered.

The chapter, "The Role of Patents in Chemical Industry," is the high point of this book, and one of the most lucid, concise explanations of U.S. Patents available. It is worth the price of the book even though it tells much more about the nature of patents than the role they play in the chemical industry. The chapter, "Marketing in the Chemical Industry," reflects the editor's field of expertness and states the broad concept of marketing embraced by the modern school, but fails to reflect authentically the importance of human influences and a host of other factors critical to a successful marketing effort. Most lacking in the chapter, "Management," are the recognition of the importance of entrepreneurship and of accomplishment of business objectives by working with people. The descriptions of the mundane aspects of business management are general and would be a good basis for discussion. For preceptive authenticity, the chapter on "Human Resources" could have been written better by the head of an industrial relations department of a major chemical company. "Mergers and Agreements" is historical and general. While dealing well with the theoretical sterile advantages of these unions, it neglects the often major factors that bring about the marriage such as the desire for negotiable stock, inheritance taxes, increased earnings per share, or the marriage being good financially for one or both of the partners. The chapters on "Advertising" and "Sources of Business Information" are well done and useful introductions. The two chapters on "Accounting" and "Financial Management" should have been combined since much common material is presented. Both chapters are elementary and useful to explain terms and functions, but much too brief to serve as more than a basis for discussion.

The book is suitable as a text for a seminar course for college science and engineering students who plan to enter the chemical field. However, it does not present a study in depth of any of the facets of "Administration of the Chemical Enterprise."—*Sherman K. Reed*

Complexing & Hydrothermal Ore Deposition by H. C. HELGESON; 128 pages; $8.50; Vol. XVII International Series of Monographs on Earth Sciences; The Macmillan Co., Pergamon, 1964.

This compact and readable little book is a welcome and timely introduction to the involved subject of complex formation in hydrothermal solutions. The author is the first to point out that this is not the final word on the subject but

is more in the nature of a progress report. It is, however, a most comprehensive progress report, with forty-eight figures, six tables, and 206 bibliographic references with entries up to 1963.

The first half of the book summarizes thermodynamic theory and data on the formation of complexes in general. Particular attention is given to methods of approximation where primary data are unavailable. A working knowledge of elementary physical chemistry is necessary in order to follow the arguments. The standard entropy of association of a complex is shown to be the most important single parameter for describing its stability and its extent of formation. Restriction to aqueous systems allows correlation of the stability of the complexes with the curves of constant entropy for water, permitting extrapolation of the calculated equilibria to more extreme temperatures and pressures. A modification of the Debye-Hückel theory is proposed for the approximate calculation of activity coefficients of individual ions in concentrated electrolyte solutions.

The theory is applied in detail to the solubility of galena in a "model ore solution" consisting of the ionic species in equilibrium with galena in the system $NaCl-HCl-H_2O$. The most important complexes contributing to the solubility of galena are $PbCl^+$, $PbCl_4^{--}$, H_2S, HCl, and $NaCl$. HS^- complexes become important only at rather high pressures of H_2S. Solubilities of from 20 to 100 ppm PbS are achieved in concentrated NaCl solutions at 125°C., and a maximum calculated solubility of 650 ppm is reported at 200°C. in 3 M NaCl solutions.

Chloride complexes are shown to be quantitatively adequate to transport ore metals and to be in good agreement with the compositions of geothermal waters and the contents of fluid inclusions. Hydrogen ion equilibrium with alteration zones suggests that reaction of the ore-forming fluid with the wall rocks may in many cases be adequate to account for the change in composition of the fluid which results in ore deposition. Changes in temperature and pressure, and contamination of the ore fluid with ground water are also shown to result in

precipitation of ore minerals carried by chloride complexes.

Sufficient detail on the method of calculation is given to enable a well-prepared reader to perform similar calculations on other systems of interest. This book is certain to provide the starting point for many similar studies of geologic interest and should provide a spur to the collection of fundamental thermodynamic data on ionic equilibria. It is highly recommended to all serious students of hydrothermal ore transport.—*Hugh J. Greenwood*

Crystal Structures by RALPH W. G. WYCKOFF; Second Edition, Volume 1; 467 pages; $17.50; John Wiley & Sons, Interscience, 1963.

This book offers many advantages over the loose leaf edition, and the publisher's claim that it is a "completely new volume" appears to be justified. Besides the obvious advantages of the book form, the usefulness of this edition as a reference file is also improved by the availability of the new name and formula indices as well as the presentation of illustrations and tables together with the corresponding text.

The book offers 20–30 per cent more crystal-structure descriptions than the looseleaf edition contained, and is augmented by additional information and revisions on many other compounds. It is clearly a remarkable work of compilation and deserves the expression of our appreciation.

Unfortunately, however, the type of information, the system of classification, and the style of illustrations were not revised to meet the changing needs of modern researchers. The data offered in the book are satisfactory to help the reader form a "first idea" on the structure; however, most investigators will need information on the reliability of the structure, temperature factors, interatomic distances, and similar crystallographic features. Although this book offers an apparently complete bibliography, the references are not identified in the text and the reader will have to check a large number of papers to find the source of the additional information. The illustrations are artistic and impressive, and illustrate the simple crystal structures adequately. They fail, however, to demonstrate the essential features of the more complicated structures, partially because the spheres in the packing models hide most of the details, and partially because the illustrations of the structures are frequently terminated at the limits of the unit cell. The adoption of other types of illustrations could have made unnecessary the author's statement on the inadequacy of the graphical presentations of complicated structures (p. 320).—*Tibor Zoltai*

Analysis & Computation of Electric & Magnetic Field Problems by K. J. BINNS & P. J. LAWRENSON; 333 pages; $12.50; The Macmillan Co., Pergamon Press, 1963.

This book is a compilation of analytic and numerical techniques for solving two dimensional problems in electro- and magnetostatics. The emphasis in the part on analytic solutions is strongly oriented toward direct solution of Laplace's and Poisson's equation and toward transformation methods of complex variables. In addition, a concluding section presents several numerical techniques particularly useful in this age of the digital computer.

What was done was well done. The authors have covered the standard material adequately and clearly, and have supplemented it with a profusion of examples. Some little-known extensions are described, particularly in the realm of Schwarz-Christoffel transformations. These have been extended to be applicable to regions exterior to finite polygonal boundaries and to approximations for rounded corners and curvilinear boundaries. A numerical method is shown for complicated Schwarz-Christoffel problems, i.e., those involving many vertices, especially when the angles are not integral multiples of $\pi/2$. An appreciable effort is devoted to finite-difference formulation of problems, and to solving these by relaxation and iteration techniques. In addition, a Monte Carlo solution is adequately covered.

Nevertheless, this reviewer's feelings about the book are mixed. It isn't fish and it isn't quite fowl, either. It is certainly not a textbook—this is not the authors' aim. As a reference book, it covers only a restricted area in the realm of electricity and magnetism, that of two-dimensional statics. With such a limitation on extent, one would expect exceedingly intensive coverage. Yet, such powerful tools as variational approaches, Greens' functions, and integral transforms are completely ignored. Even multipole expansions are not mentioned. Granting that these are primarily three-dimensional methods, they are nevertheless applicable to two-dimensional problems and should at least be mentioned in a handbook of two-dimensional techniques. The best use of this book will probably be as a reference on the otherwise well-equipped bookshelf, and possibly as a supplementary text in a first graduate course. —*A. J. Estin*

McGraw-Hill Yearbook of Science & Technology (1963 Review; 1964 Preview), D. I. EGGENBERGER Managing Editor; 438 pages; $22.50 or $13.50 to subscribers to the *Encyclopedia;* McGraw-Hill Book Co., 1964.

This 1964 Yearbook is the third annual supplement to the McGraw-Hill Encyclopedia of Science and Technology. It contains the expected collection of brief articles describing important technical work in 1963. In addition, there are nine longer articles on such diverse topics as the biology of fertility, reform of high school science, years of the quiet sun, and the electronic revolution.

An encyclopedia is basically a collection of facts, and this yearbook is no exception. The reader may learn all sorts of things, of which the following represent a tiny sampling.

Spun protein fibers from soy beans are now textured into foods that look, taste, and eat like meat products, not as flavorful as their real counterparts, but good imitations. The Russians seem much more concerned with radiation hazards than are we, and this may be

the main reason for their lack of interest in landing a man on the moon. The average life span of cells in the taste buds of vertebrates is only 200 to 300 hours. The weather satellite Tiros VI, launched September 18, 1962, photographed tropical storms Claudia, Emma, Freda, Gilda, Daisy, Jean, Karen, and Lucy.

One kilowatt-hour of electricity is now being generated from 0.85 pound of coal, on the average. Sexual behavior is primarily dependent upon activity of the central nervous system. A researcher has formulated a mathematical model based on this idea which yields predictions closely supported by experimental data. One somewhat novel sentence: "The fewness of helicopters being used for transportation contributes significantly to their over-all speed."

In all seriousness, however, the McGraw-Hill Encyclopedia is too well known to need favorable comments for its coverage of technical matters. This volume carries on in providing timely supplementary material.—*W. J. Cunningham*

The Speech Chain: The Physics & Biology of Spoken Language by P. B. Denes & E. N. Pinson; 158 pages; $.75 each for 10 to 24 copies; Bell Telephone Laboratories (The Williams & Wilkins, Co., Baltimore, Md., distributor), 1963.

Denes and Pinson have written an excellent elementary textbook that stresses the interdisciplinary character of present-day speech science. It is aimed at advanced high school students, but it can perhaps be used more profitably on the college level. The authors' intimate awareness of, and participation in, recent research is apparent even in the general thoughts offered in the introductory chapter.

The role of linguistics as the discipline that furnishes the theoretical framework for much phonetic research does not emerge clearly, but the authors do convey the notion that there are important linguistic constraints on the perceptual processing of the speech signal. The sections on the anatomy and physiology of speech and hearing are clear and straightforward, omitting nothing essential, but the speech "major" will have to do considerable supplementary reading to gain a knowledge of muscles and their functions.

The authors lucidly present the basic principles of acoustics, with emphasis on those needed for the study of speech. No sophistication in mathematics and physics is assumed on the part of the reader. The concept of resonance, crucial to an understanding of the transmission properties of the vocal tract, is briefly but clearly set forth. The latter part of the book is devoted to a survey of the acoustic characteristics of speech. The authors then go on to describe how the experimental manipulation of both natural speech and synthetic speech is used in the psychoacoustic testing of hypotheses on the acoustic cues to speech intelligibility.

The slimness of the volume and the clarity of the style tend to mislead students into underestimating the contents. Yet, if, upon graduation, the average student of speech therapy or audiology were truly in control of the contents of this little book, he would have far stronger disciplinary underpinnings to his professional training than he typically seems to have today. —*Arthur S. Abramson*

The Language of Nature (An Essay in The Philosophy of Science) by David Hawkins; 372 pages; $7.50; W. H. Freeman & Co., 1964.

As the reader may have guessed, the language mentioned in the title is mathematics. The first nine chapters, five sevenths of the book, give a rather extensive survey of the structure of principles and axioms in many if not most fields of mathematics and mathematical physics and discuss some of their epistemological significance. This ranges from number theory, set theory, and formal logic over some geometry, kinematics, dynamics, and more modern theoretical physics, to statistical problems: probability theory, statistical thermodynamics, information theory, and the theory of induction. In all of

these the reader will find an easy-to-read account touching some of the high points, eschewing formal apparatus, and emphasizing many aspects of conceptual or philosophical significance. The breadth of coverage is remarkable if not unique for so small a book, and the amount and variety of stimulation toward thought and further knowledge which a student or spare-time student with some native interest in mathematics can glean from this volume is extraordinarily large, such as can usually be had only from far heavier and more technical tomes. The author has an excellent flair for simplifying the intricacies of mathematical reasoning.

On the other hand, the author is heavily preoccupied with mathematical modes of thought. Nature seems to mean to him something already abstracted; the idea that the term can also refer to the interaction of humans with concrete reality never occurs. The last two sevenths of the book apply the same abstractionist approach rather less successfully to other realms of experience, as may be indicated by the headings of Chapters 10–13: Three Evolutionary Stages; The Theory of the Soul; Ethics; Economic Theory and Social Choice (all in 100 pages).

The illustrations, by E. L. Gillespie, are sensitive and delightful; this, the glossy paper, and the substantial binding seem designed to make this essay more an item for the library of a member of the propertied classes than for the student to whom the author appears to address himself.—*Walter M. Elsasser*

Thin-Layer Chromatography by K. Randerath, translated from the German by D. D. Libman; 250 pages; $8; Academic Press, New York, 1963.

In 1938, two Russians, Ismailov and Schraiber, described the use of thin layers of adsorbents spread on glass plates in separations analogous to adsorption chromatography on columns introduced by Tswett in 1903. The method received scant attention until Stahl and others, primarily in Europe, developed it as a general method of analytical chromatography adapted to

the use of partition and ion-exchange media in addition to the original adsorption materials. In this book, Randerath presents a detailed discussion of the technique of thin layer chromatography, its advantages and disadvantages in comparison with other chromatographic processes, and the numerous applications of the method which have been developed.

In the first of two sections, after an introduction, the author discusses chromatographic theory with most attention to adsorption processes; this is brief but well done. There follow detailed descriptions of techniques including preparation of layers, different procedures for developing chromatograms, evaluation of the chromatograms, thin layer electrophoresis, permanent recording of the chromatograms, and preparative thin layer chromatography. These considerations are complete and serve as an excellent introduction to the general method.

The second section, devoted to special applications of thin layer chromatography, is divided into 15 parts based on rather broad chemical classes. As examples, alkaloids, vitamins, steroids, antibiotics, amino acids, nucleic acids, carboxylic acids, and plasticizer esters are treated, among many others. Each part contains information on visualizing the chromatograms, use of different layer materials, and the Rf's of selected substances in a variety of developing solvents; reference to the original literature is extensive. This section contains a wealth of information on specific separations using thin layer chromatography and will be of value to anyone with separation problems.

Overall, this book is an excellent introduction and complete practical guide to the art of thin layer chromatography as it is now practiced. The literature citations are in the German style whereas the Chemical Abstracts system would have been more appropriate in the translation. The translator is also guilty of some contorted syntax and unidiomatic English. However, these criticisms are minor. The book is highly recommended.—*David Strauss*

Nuclear Chemistry by N. R. Johnson, *et al.;* 202 pages; $8; John Wiley & Sons, Interscience, 1963.

This is Volume II of the "Technique of Inorganic Chemistry" series. It is neither a textbook nor a laboratory manual, but is intended to be a survey of those experimental techniques which are useful in nuclear chemistry and tracer work. There are three types of scientists for whom such a book might be written: The graduate student starting to do research, the active researcher who has never done "radioactive" work but is willing to start now, and the working nuclear chemist. For the first two groups this volume will prove to be a valuable introduction and guide. The theoretical material which is relevant to an understanding of experimental techniques is presented clearly and concisely. The most important sections deal with separation techniques and with detection and measurement of nuclear radiations, and these are especially recommended. In the latter section, particular emphasis is given to scintillation counters and semiconductor detectors. The book is less useful for a worker in the field, especially one who needs information about electronics, since the "black box" treatment is used to describe electronic systems.—*Sheldon Kaufman*

Vacuum Technology by A. Guthrie; 532 pages; $12.50; John Wiley & Sons, 1963.

The stated purpose of this book is to present a set of working rules primarily for the technician who sets up, operates, and maintains vacuum systems. The author achieves this goal admirably and is the first to do so. The appeal of the book, however, will be to a wider audience, for all who use vacuum systems should benefit from the vast amount of practical know-how presented.

After a very cursory treatment of the properties of gas at low pressure, the components, materials, and processes of vacuum systems are covered in some detail. The emphasis is primarily on how to do it. Clear and concise line drawings of vacuum components illustrate their

operation. Many tables and graphs make design information readily accessible. Lists of advantages and disadvantages of various components and processes provide concise summaries. A large number of sometimes trivial numerical examples are given to illustrate component and system operation.

While the measurement of pumping speed is treated in some detail, the equally important subject of gauge calibration is barely mentioned. Treatment of ultrahigh vacuum procedures is a bit shallow.

In conclusion, this down-to-earth treatment of vacuum practices should find ready acceptance among those who *use* vacuum systems.—*Richard W. Roberts*

Theory of Linear Physical Systems (Theory of Physical Systems from the Viewpoint of Classical Dynamics, including Fourier Methods) by E. A. GUILLEMIN; 586 pages; $12.50; John Wiley & Sons, 1963.

Considering the subject matter of this text, a more appropriate title for Professor Guillemin's most recent book would be "Frequency Domain Techniques for Linear Networks." In a recent book on linear physical system theory this reviewer expected to find material on state space techniques for linear system analysis as well as frequency domain subjects. This book is essentially a discussion of frequency domain theory with applications to linear networks. It is written in the style for which Professor Guillemin is well known from his earlier books on network theory. The emphasis on network topology and rather thorough treatment of Fourier Methods enhance the value of the text to the serious student. The discussion is very strongly oriented toward electrical network applications.

There are eighteen chapters, some of which are entitled: Solution of Algebraic Equations When Auxiliary Conditions Are Specified, Synthesis of Networks from Given Parameter Matrices, Power and Energy Relations, Consideration of Linear Active and Nonbilateral Elements, Approximation and Convergence Properties of the Fourier Series or Integral, Sampled and Band-limited Functions, and Hilbert Transforms.

The book contains sufficient material for an advanced senior class or first year graduate class in electrical engineering. Because of the constant emphasis on networks, the book probably will be of most value to electrical engineering students and workers. It would make an excellent and thorough review for practicing engineers in circuit design and analysis if it could be studied diligently. Professor Guillemin writes books which cannot be read casually and his most recent work is no exception.—*Stephen J. Kahne*

Physical Properties of Soils by R. E. MEANS & J. V. PARCHER; 464 pages; $11.25; Charles E. Merrill Books, Inc., 1963.

This is an elementary text on the properties of soils as they are of interest to the foundation and construction engineer. From an introductory discussion of the atomic structure, interatomic bonds, and the resulting crystal systems, the authors move quickly to the definition and discussion of such soil engineering properties as grain size distribution, plasticity, structure, permeability, compressibility, and shear strength.

These properties are defined clearly and are discussed at great length in a descriptive and qualitative fashion. This will be an advantage for all those who wish to get a quick introduction to the concepts used in soil engineering and a basic understanding of the important phenomena that influence soil properties and soil behavior. A final chapter discusses laboratory procedures for determining each property.

It is the stated conviction of the authors that the successful practice of foundation engineering depends more on a firm understanding of these soil properties—their determination, interpretation, and significance—than on techniques of engineering mechanics. Thus, the quantitative, analytical treatment offered is scant. A demonstration of how

the properties and concepts discussed are applied to actual problems of analysis and design is completely absent. This may be one of the weaknesses of the text. The authors rely heavily on notes taken from the lectures and teaching of Arthur Casagrande of Harvard during the academic year 1938–39. They unashamedly acknowledge this origin and state that properly such a book should have been written by Casagrande himself. Consequently, a good part of the text is "classical" subject matter although most chapters have been brought in line with modern thinking. Some of the material is clearly dated, e.g., the consolidation testing frames; other recent developments, e.g., in soil sampling tools such as the Swedish Soil Sampler, have been ignored.

Although the evaluation of the laboratory data is often discussed in great detail a proper assessment of experimental errors—systematic or incidental—is lacking and this is a regrettable shortcoming. Reproducibility of data and the significance of errors should be treated thoroughly, if such emphasis is to be placed on the determination and interpretation of properties. For such a purpose it would have been necessary to show the use of data in the mathematical solution of analysis and design problems encountered in practice. This apparently was beyond the scope of the book.

Uncertainties, data variations, and experimental errors are, however, important facts of life for any engineer and especially for those in soil mechanics. A demonstration of how to assess the properties measured could have made the text more valuable and instructive.—*Werner E. Schmid*

Volatile Silicon Compounds by E. A. V. Ebsworth; 179 pages; $7.50; The Macmillan Co., Pergamon, 1963. (Vol. IV of International Series of Monographs on Inorganic Chemistry).

This is an entertaining book. Odd and unexpected information pops out at the reader, along with an excellent and concise summary of the behavior of the volatile compounds of the Group IV elements. There is a great deal about quadrupole coupling constants, force constants, paramagnetic terms, and excited-state mixing. The book is mostly a discussion about why chemical bonds to silicon and its congeners are as long or as short as they are, and why a reaction follows Path A rather than Path B.

This is an informal and chatty book. It is purposely full of personal opinions, such as: "Electronegativity is one of those irritating concepts like covalent character which sound as if they mean very much more than they do." Later, regarding the behavior of substituted silanes toward standard reagents used to deduce the effect of substitution at silicon; "Unfortunately this is usually a pious hope rather than a profitable exercise." Some of the opinions are backed up by a unique and extremely helpful summary of descriptive chemistry, as in the excellent chapter on the silicon-hydrogen bond, which is replete with the latest measurements of coupling constants and chemical shifts (many of them heretofore unpublished). The author comes to the conclusion that "There is no general correlation between chemical shift and vibration frequency for SiH bonds, although in certain series of compounds it may be possible to discover empirical relationships between the two properties." In other instances the firm opinions of the author are refuted by his own facts. For example, in a discussion of the bond lengths in the methyl compounds of silicon, germanium, and tin, the author states that "Although the precise values for the M-C bond lengths obtained from the monomethyls do not agree very well with the sums of the appropriate covalent radii, the agreement is greatly improved by the Schomaker and Stevenson electronegativity correction." Actually, the agreement is improved in one case (Si) and worsened in the other two (Ge and Sn), as shown in Table 4.1 As for the statement "Germanium (II) is rather ill-defined," (p. 4) this will be news to the international suppliers of germanium who offer GeF_2, $GeCl_2$, $GeBr_2$, and GeI_2 as articles of commerce, and can furnish GeO 99.999 per cent pure.

This will be a controversial book. The author chooses to ignore recent Russian books like Petrov, Mironov, Ponomarenko, and Chernyshev, and sticks to his own sources. Your reviewer made a long list of points on which he himself differs sharply with Ebsworth, and another list where he complains that Ebsworth simply slides over facts such as the reaction of organolithium compounds with GeH producing GeLi and RH whereas SiH produces SiR and LiH. Some readers will also find fault with the English: *Liable* is used for *likely*, *if* instead of *whether*, and *disproportionation* instead of *condensation*. The author also is given to anthropomorphic usage: "silicon prefers," "iodosilane appears reluctant," and so on. This is conversational usage today, and it flows naturally into the book. At the same time, American editors and teachers decry this sort of thing. In the days of Pygmalion the British were the guardians of the language; now there is a stronger rear-guard action being fought over here, with just as little success.

This is a good book and an important one, so practically everyone in inorganic chemistry will want to own it. Each reader will find his own stimulation in it. Chemistry is so vast and diffuse these days that no one man can know *all* the facts about a field, much less comment on those facts. Yet Ebbsworth comes closer than anyone else to accomplishing just that for the volatile compounds of silicon. The book is recommended.—*Eugene G. Rochow*

The Precambrian, Vol. I of *The Geologic Systems*, edited by K. Rankama; 279 pages; $8.95; John Wiley & Sons, Interscience, 1964.

The editor states that this is the first of a series of volumes on the Precambrian which are to present comprehensive regional surveys that will emphasize the classification, subdivision, and correlation on the basis of exact ages, but that other geologic aspects necessary for achieving a well-balanced picture of the Precambrian of a given country have not been forgotten. The volume contains an introduction by

Arthur Holmes that outlines the history and emphasizes the significance of the development of methods and use of "radiometric ages" in deciphering geologic history. The region covered by this volume is the Baltic or Fennoscandian shield and the respective authors are Arne Noe-Nygaard for Denmark, T. F. W. Barth and P. H. Reitan for Norway, Per Geijer for Sweden, and Pentti Eskola for Finland. All the authors are outstanding in their knowledge of their subject and it is fortunate that they have been willing to cooperate in this venture.

A reading of this book shows how great has been the contribution of "radiometric ages" to our knowledge of geologic history but it also emphasizes how necessary is an understanding of the geology to the appropriate interpretation of the *significance* of the *numbers* obtained as "radioactive ages." Similarly, geothermometric data are but numbers whose significance must be evaluated in the light of geology. For example, the temperature (450°C.) given on pages 41–42 for anatectic

(differential melt) granite need not imply the temperature of the melt but could represent a subsolidus autometamorphic recrystallization.

The usefulness of this book will be primarily as an authoritative reference work where an up-to-date summary and appropriate list of references may be had for the Precambrian geology of the region discussed. Indeed, it should be noted that it is actually *general geology* that forms the greatest part of the volume and that classification, subdivision, and correlation on the basis of "exact ages" is subordinate, as it needs must be in the present state of development of this field of research.—*A. F. Buddington*

Life Stress & Mental Health by T. S. Langner & S. T. Michael; 517 pages; $9.75; The Macmillan Co. (The Free Press of Glencoe), 1963.

This second volume from the Midtown Manhattan Study traces relationships between childhood experiences and adult mental health in the lives of a random sample of 1660 midtown New Yorkers.

The expected finding that mental illness shuns the socially advantaged leads to a discussion of how the middle class and the poor chose different ways of handling stress, so that similar troubles produced unequal effects. Parents did seem to "teach" their children mental health; quarreling parents, especially, scarred the minds of their young. Broken homes, contrary to an old psychiatric belief, were not clearly related to the child's developing poor mental health. Trouble appeared when children, for whatever reason, came to hold a negative view of their parents.

Fascinating details abound in a book jam-packed with data. The reader will find it heavy going unless he reads for continuity; the thread of the text is a hypothetico-deductive teasing out of meaningful interactions among some 468 variables. Both lay and technical readers may wish the authors hadn't seen fit to invent a statistic all their own—christened the "ridit"—that can be hard to interpret. How low are these statistically "significant" correlations expressed in ridit form?

Uneasiness about how much to trust self-reports about emotions and uneasiness about a mental health criterion no stronger than a rating are shared with the reader by the authors, but not allayed. Yet, the data having confirmed his expectation that both private and clinic patients are atypical of the population of the mentally ill, the reader may prefer understanding the causes of mental health in terms of this well-sampled data to thinking on the basis of none. Whether he feels we have gained enough understanding to predict mental health with confidence, or, beyond that, to control public mental hygiene, is a decision each reader is likely to reach for himself.—*Charles C. McArthur*

Linear Programming & Extensions by G. B. Dantzig; 625 pages; $11.50; Princeton University Press, 1963.

Linear programming is a discipline of mathematics which, ironically, has little to do with the programming of computers. A satisfactory description of it is presented on the fly leaf of the book under review, wherein it is said that "The basic theme is the solution of linear inequalities—a modern constructive theory rooted in the classical approach to linear-equation systems and its related matrix algebra."

In presenting the specifics of this field and its outgrowths, Dantzig has done a near-exhaustive job. From the historical chapter on Origins and Influences, to the ones on vector-matrix theory, on game theory, on convex programing, and on integer programing (to mention just a few) the author has presented the work in truly remarkable detail. Illustrative examples are many and varied, and the bibliography list is extensive (although, sadly, containing no true entries past 1961).

If faults were to be found in this book, there would be only two. The first is the author's unerring devotion to thoroughness in those aspects of the theory which he has presented. Often, where one numerical example might suffice, he presents two; where two explanations

of the same concept might suffice, he presents three. The result is a book which is virtually self-contained, and one which is quite pleasing to the reader who knows little or nothing of the subject. However, such a treatment leaves this reviewer with the impression of proceeding through the topic with a learning machine, rather than with a concisely written text or reference book. To the person who requires a book on the field for his day-to-day work, some of the smaller, less expansive ones previously published might be more suitable.

The second fault in this otherwise fine book is the lack of a presentation on the electrical analog of linear programming. Outside of an abstract approach in terms of topological concepts, nothing is said of this interesting and important facet of the theory, which can lead to the solution of linear programming problems by analog computers.—*Walter J. Culver*

The Ecology of Waste Water Treatment by H. A. Hawkes; 203 pages; $5; The Macmillan Co., Pergamon, 1963.

Although the author calls his book an ecological study, it functions best as a physiological description of waste treatment processes. In this role it is a model of clear, direct, practical accounting of what goes on in trickling filter systems and activated sludge treatment under different loading and operating programs. It is so good that it is useful to a plant operator. My recommendation would be that the first three chapters, which represent the sort of Zoo. 1 and Elementary Biology lectures that graduate sanitary engineers get in this country, be eliminated. The remaining four chapters would make a small book for the operator's coveralls pocket and for the consulting engineer's briefcase.

Professor Hawkes has neatly trimmed away the holy verbiage that clouds the technical literature of waste treatment and has arranged the salvage into understandable good sense. It is unlikely that other-directed biologists will be enthusiastic, but it will make delightful reading for men actively involved in

waste abatement operations. Although the author claims a special interest in the microbiology of trickling filter systems, I regard his chapter on the design and operation of activated sludge systems as the most understandable and useful synthesis in the current literature. There are limits to what a biologist can say to a sanitary engineer or waste specialist, but Professor Hawkes has managed to get his ideas across by (1) getting experience with the processes, and (2) talking in terms of what he has learned from the engineers and operators.—*Charles E. Renn*

Organic Functional Group Analysis by F. E. CRITCHFIELD; 187 pages; $6.50; The Macmillan Co., Pergamon, 1963.

Written essentially as a laboratory manual for analytical chemists, this work is inherently drastically limited in scope. Consequently, it is not a book which any organic chemist will find a valuable addition to his library. It is to the author's credit that he is not only aware of most of these limitations, but actually makes an effort to point them out. As the author notes, the methods outlined in this book will be found particularly valuable for: "(1) the determination of the purity of refined materials; (2) the determination of ppm concentrations of organic compounds; (3) the analysis of research samples; (4) the calibration of instrumental methods; and (5) the rapid analysis of process samples." This reviewer would question the third application in this list, except as it may be a special case of the first.

The material presented consists of a limited number of "versatile and reliable" methods of quantitatively analyzing for a variety of simple organic functional groups. No effort is made to review either critically or comprehensively the literature on this topic; rather, the methods presented are limited to those which have been developed, modified, or used routinely in the author's laboratory. Although the limitations and scope of each method are discussed in some detail, the reader is offered virtually no assistance in seeking out alternate methods. Given all of these self-limitations, the author presents his subject concisely and with clarity and precision. In short, this book can be recommended as a supplemental, but not definitive, procedural manual for any chemist who must routinely perform one of the tasks outlined in the preceding paragraph.—*G. Dann Sargent*

Tables of Experimental Dipole Moments by A. L. MCCLELLAN; 713 pages; $14; W. H. Freeman & Co., 1963.

Since there have been no recent publications which bring together all the reported experimental dipole moments, this compilation is a timely addition to the reference shelves. The arrangement and the description of the tables are excellent and make the values readily available to even a casual user. The cross-indexing of authors and the cross-reference list of metal-organic compounds are quite helpful.

The reader must obviously be cautious about accepting any one value in such a collection of non-evaluated data. Values for metal-organic compounds in particular must be considered as only indicative unless they have been well substantiated. The situation is somewhat improved by a table of average dipole moments found for some common molecules and by a table comparing average values for a number of homologous series, although an average does not necessarily give a good value.

For fifty-two of the roughly 6000 compounds covered, "recommended values" of the dipole moments are given, based on a recalculation of experimental data. Since the compounds were chosen for this reevaluation on the basis of having a large amount of consistent data from several authors, the resultant dipole moments would be expected to be quite reliable. However, carbon dioxide is given a recommended dipole moment of 0.18 debye, in spite of the fact that it is known to be non-polar. No explanation of this disparity is given. Since the other compounds thus treated are polar, similar experimental errors are neither so obvious nor so critical.

These tables will save both the specialist and the novice considerable time

and effort in locating experimental dipole moment values and will provide some assistance in choosing the best value in a set.—*Ralph D. Nelson, Jr.*

The Chemistry & Physics of Rubber-Like Substances, edited by L. BATEMAN; 784 pages; $25; John Wiley & Sons, 1963.

Twenty-eight past and present staff members of the Natural Rubber Producers' Research Association have contributed to this book 19 reviews in selected areas of the chemistry and physics of rubber. The topics included and their extent of coverage are indicated by the following chapter titles, with pages per chapter given in parentheses: Chemistry of Vulcanization (111); Oxidation of Olefins and Sulfides (65); Abrasion and Tire Wear (60); Stress-Relaxation Studies of Network Degradation (51); Strength of Rubbers (49); Graft Copolymers from Natural Rubber (36); Viscoelastic Behavior (36); Correlation of Vulcanizate Structure and Properties (31); Structure, Composition and Biochemistry of Hevea Latex (30); Theory of Rubber-like Elasticity (30); Mastication and Mechanochemical Reactions of Polymers (29); Radiation Chemistry (27); Effects of Fillers in Rubber (26); Ozone Attack on Rubbers (23); Colloidal Properties of Latex (21); Crystallization in Natural Rubber (21); Cis-Trans Isomerism in Natural Polyisoprenes (20); Theory of Rubber Solutions (18); Natural Rubber Hydrocarbon (15).

This book should be the definitive work on the fundamental research aspects of rubber for many years to come. The novice will find the book useful since the authors have emphasized the general principles and experimental results in their areas by the abundant use of figures, tables, formulae and references. The longer chapters will be helpful even to research experts in these fields.

The book is not a completely general review in that the contributions of the staff of the N.R.P.R.A. have been emphasized throughout. The almost complete concentration on natural rubber suggests that "The Chemistry and Physics of Natural Rubber" would have been a more fitting title.—*K. W. Scott*

Molten Salt Chemistry, edited by M. BLANDER; 755 pages; $25; John Wiley & Sons, Interscience, 1964.

This book gives an account of fundamental principles and developments in molten salt chemistry, largely due to the advances in this area in the last two decades. As the first monograph on this subject, this work will be of interest as an authoritative source to scientists in both academic and industrial research centers. Blander, as editor, has assembled a first-rate collection of contributions covering the various aspects of theoretical and physicochemical studies in molten salt chemistry. The authors contributing are: S. H. Bauer, M. Blander, M. A. Bredig, M. D. Danford, D. W. James, K. E. Johnson, A. Klemm, H. A. Laitinen, H. A. Levy, C. H. Liu, R. F. Porter, J. E. Ricci, G. P. Smith, and F. H. Stillinger. The strong representation from the Oak Ridge National Laboratory reflects directly the stimulus to this area of research through the U.S. Atomic Energy Commission.

The subject is developed in ten chapters, each contribution being a critical review, with bibliography. The first, on the equilibrium theory of fused salts, gives an assessment of the approximate theories (significant structure, hole, and lattice theories) and develops current viewpoints from the ion pair correlation functions and statistical theory; the second chapter, on X-ray and neutron diffraction studies, critically examines this approach for fused salts and the results in this area of structure studies. Molten salt solutions, starting with common-ion effects and continuing with systems of increasing complexity from the thermodynamic viewpoint are discussed in the third chapter; corrections for non-random mixing and generalized quasi-lattice calculations are clearly discussed. The fourth chapter on phase diagrams develops the "comparative anatomy" of salt systems; useful classifications, ranging from simple eutectics to quinary and higher systems, and

liquid immiscibility are found here. Excellent accounts of solutions of metals in fused salts, and the structural studies based on electronic absorption spectra and vibrational spectra follow in the next three chapters. Transport properties of molten salts are considered in the eighth chapter, with emphasis on phenomenological relations as distinct from quantitative correlations by means of microscopic models. The compilations of viscosities, conductances, diffusion coefficients, and ionic mobilities were much needed and are found welcome. The structure and thermochemistry of metal halide vapors in relation to fused salt chemistry are developed in the ninth chapter, and electroanalytical chemistry, including reference electrodes and electrode kinetics, in the tenth chapter.

The treatments are authoritative; attention is directed more to theoretical viewpoints and results, as distinct from experimental techniques or applications in technology.

Among the few errors noted were the omission of the subscript 2 for $HgCl_2$ on p. 575 and the wrong subscript for $TlNO_3$ on p. 577. A thorough subject index has not been completely realized and the reader will have to search vigorously on certain topics to gain the maximum from text. Two illustrative examples are: (1) the actual reference to an experimental determination of the dielectric constant for a fused salt is on p. 525, but is not indexed, and (2) the Zarzycki "microstructure" model for halide melts, assessed in some detail on pp. 121–122, has not been included in the subject index.

This book is a "first" in the general area of fused salt chemistry; it is seen as an important reference source and an authoritative treatment. I recommend it to all scientists in industry and universities who are active or interested in fused salt chemistry.—*George J. Janz*

Geophysics, The Earth's Environment, edited by C. DEWITT, *et al.;* 624 pages; $10.50 cloth; $8.50 paper; Gordon & Breach, 1963 (Lectures Delivered in French and English at University of Grenoble's Theoretical Physics Summer School, Les Houches, 1962).

This volume, like the others emanating from Les Houches, is a very useful compendium for students of the physics of the upper atmosphere. The contributors are drawn from among the most distinguished specialists in their respective fields. At their best, the lectures constitute authoritative and surprisingly comprehensive monographs. For example, Budden's eighty pages on magneto-ionic theory is among the best treatments the reviewer has seen. The elementary aspects are reviewed concisely, and advanced topics such as the impulse response of the ionosphere are treated in some detail.

Nicolet's lectures on the composition of the upper atmosphere constitute a rich storehouse of data on the photochemical processes there. It is regrettable that this contribution is alone in having no bibliographical references whatever, even though it must have been drawn from hundreds of sources.

Denisse's contribution on plasma physics is noteworthy for its proof that Landau damping does not exist. Chamberlain gives an excellent review of Störmer theory and the theory of adiabatic invariants. Benoit shows that Denisse's contentions on the existence of Landau damping can be tested by ionspheric scattering experiments. MacDonald's lectures on atmospheric tides are worth reading, as are his lectures on the elementary theory of the ionosphere. Barbier reviews high-altitude luminescence authoritatively. Gallet's discussion of whistler dispersion nicely complements Budden's article.

Dungey's article, entitled "The Structure of the Exosphere, or Adventures in Velocity Space," is unsystematic and plagued with a host of typographical errors—the only ones found in the book. These deficiencies are remedied by a fascinating discussion of the topology of the magnetic field resulting when the terrestrial dipole is immersed in the magnetized solar wind.

Taken as a whole, the book is a relatively systematic and well-arranged compendium. The book making is unexceptionable.—*George B. Field*

Charles Darwin by SIR GAVIN DE BEER, 290 pages; $4.95; Doubleday & Co., Inc., 1964.

There has been so much written about Darwin and Darwinism lately that I would have thought it quite impossible to find any pleasure or profit from a new book called *Charles Darwin,* even though it is written by de Beer who has had so many worthwhile things to say, from Hannibal's route across the Alps to a classic discussion of embryology and evolution. But fortunately, my fears were totally without foundation for the book has an exceptionally large number of good qualities, put together in just the right combinations.

It is a short book written with the utmost simplicity. The parts about Darwin as a person have the same direct tone as Darwin's own short autobiography. Added to this frame there are the scientific contributions of Darwin in geology, in evolution, in the expression of emotions of animals, in plant physiology, plant reproduction, and in soil transformation and enrichment. Not only are each of these critically examined and explained in terms of knowledge, but, for each, the train of thought is followed from their first beginnings in Darwin's notebooks. One can therefore see the man, the beginning and the progress of the ideas within him, and their significance to the advance of biology. The straightforward style and the compactness of the whole book provide a special kind of understatement that has an electric effect: the magnitude of Darwin's scientific achievement not only in the field of evolution, but biology as a whole becomes staggering. One cannot finish reading this book without being fully infused with the idea of Darwin's genius.

Besides its general excellence the book does also have a novelty. By carefully consulting Darwin's many notebooks and letters, new insights and new thoughts other than those from his published work emerge. Many of these are illuminating in his own development, but others are remarkably prophetic in terms of what we know today. For instance, we are all familiar with S. L. Miller's recent experiments on the production of amino acids by the use of an electric arc in the presence of methane, ammonia, and water vapor, all of which were postulated by H. C. Urey to be present on the surface of the earth at an early period of geological history. In 1871 Darwin wrote "... But if (and oh! what a big if!) we could conceive in some warm little pond, with all sorts of ammonia and phosphoric salts, light, heat, electricity, etc., present, that a proteine compound was chemically formed ready to undergo still more complex changes ..."

As de Beer points out, Darwin anticipated here by eighty years. There is, in fact, only one important place where Darwin failed and that is on the mechanism of heredity. He needed and understood the need for the answer much more than Mendel yet he did not find it himself, or even find Mendel's published results. If he had had this key bit of information he undoubtedly would have been the father of the "new synthesis" as well; but perhaps it is good that there were some limits to his genius, and that there are some things left for us to do.—*J. T. Bonner.*

Von der Brutfürsorge heimischer Spinnen by J. PÖTZSCH; Die Neue Brehm-Bücherei, No. 324; 104 pages; 9.80 DM; Wittenberg Lutherstadt: A. Ziemsen Verlag, 1963.

Books on spiders are few and far between. It is therefore a pleasure to report on a new one in the series of booklets of Die Neue Brehm-Bücherei. The little volume mainly describes the care of eggs and young of central European spiders, most of them representing genera found also in North America. It is a mine of information on development, eggs, eggsacs, and brood behavior.

The booklet is illustrated by 78 excellent black and white and ten colored photographs, many unique, showing various aspects of brood behavior. It is complete with references and index. One wonders why it was considered desirable to change the name of the common triangle spider, known for almost 100 years as *Hyptiotes,* to *Uptiotes.*—*Herbert W. Levi*

INDEX TO BOOKS REVIEWED. *This index is for quick reference to books reviewed in this issue. In light of the pressure on space in THE SCIENTIST'S BOOKSHELF section, reviewers are urgently requested to bear in mind that, normally, the length of reviews should be about 300 words.*

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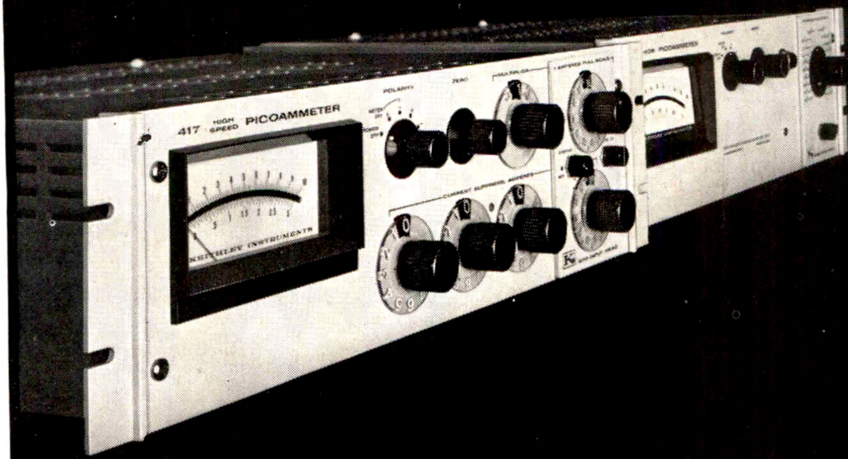

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DECEMBER, 1964

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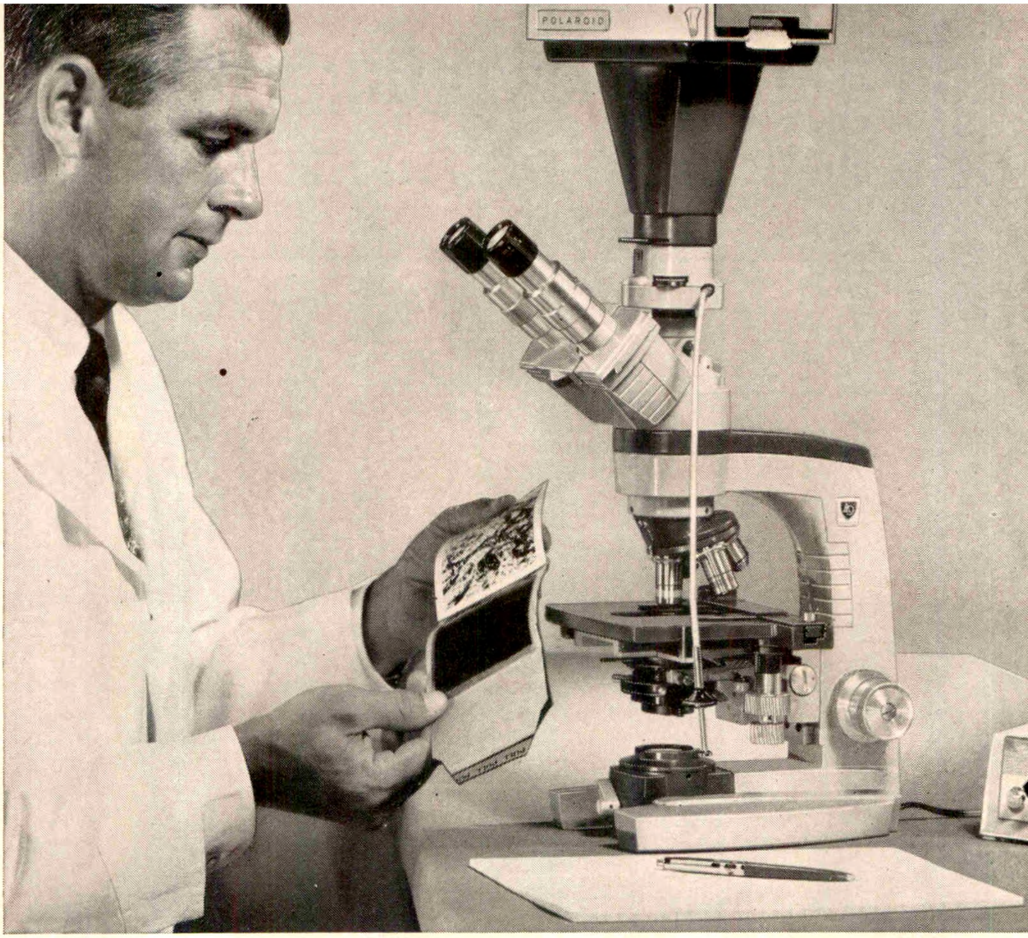

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VOLUME 52 DECEMBER 1964 NUMBER 4

PUBLISHED IN THE INTEREST OF SCIENTIFIC RESEARCH
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CONTRIBUTORS

Through the courtesy of the officers of American Philosophical Society of Philadelphia, we are permitted to reprint the lecture of the Vice-President and Director of Research, General Electric Company, Schenectady, N.Y., given at the April 1964 meeting of the Society and printed in the Proceedings in October 1964. Dr. Suits, recently a member of the Executive Committee of the Society of the Sigma Xi and a William Procter Prizeman of RESA, here outlines the scientific development which made practical the production of industrial diamonds and also the parallel researches on boron-nitrogen solid compounds.

*Trademark of General Electric Co.

The author obtained his undergraduate and graduate training, respectively, from the University of Toronto in biology, and from the University of Chicago in zoology. He has spent the last decade in basic research on resource management in fisheries and forestry for Canadian governmental and provincial agencies. Currently he is Associate Professor of Zoology in the University of California at Davis. His essay indicates the values of computer simulation in the very complex problems that prevail in all areas of resource management. He makes a plea for the specialized training of personnel for such objectives.

A Professor of Mathematics and Chairman of the Department of Mathematics at the University Heights Campus of New York University since 1959, was a graduate of Oberlin in 1931, a Rhodes Scholar at Oxford, 1934, and a Ph.D. of Princeton in 1938. His teaching experiences include an instructorship at Cornell, 1938–1942, and a professorship in the University of Tennessee, 1942–1959. He is serving a term of office as Editor of *American Mathematical Monthly*, 1962–1966. In the summers of 1962 and 1963 he was respectively Visiting Professor at the University of Washington and the University of California at Berkeley. He was a member of the Operations Evaluation Group of the U.S. Navy, 1944–1945, and a consultant of Union Carbide Nuclear Co., Oak Ridge, since 1946.

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Second-class mail privileges authorized at Easton, Pa. Additional entry at the New Haven, Conn. post office, December 9, 1943. This publication is authorized to be mailed at the special rates of postage prescribed by Section 132.122.

LYLE B. BORST, *Liquid Helium* 431

The unique properties of liquid helium are here set forth by a Sigma Xi-RESA National Lecturer. A physicist with a doctorate from Chicago in 1941, he has specialized in nuclear physics working in Chicago on the uranium chain reaction, and supervising reactor research at Oak Ridge, 1943–1946. An original member of the Brookhaven National Laboratories Staff he was concerned with reactor design. He returned to teaching in 1951, successively at the University of Utah, New York University, and most recently at the State University of New York at Buffalo, N.Y. His recent researches on liquid helium stem from neutron studies of molecular crystals at liquid helium temperatures. He has demonstrated the existence of ortho and para forms of water, acetylene and methane, paralleling the earlier ortho and parahydrogen. Address: Department of Physics, State University of New York at Buffalo.

D. E. BERLYNE, *A Decade of Motivation Theory* 447

A graduate of Cambridge University, with a Ph.D. degree from Yale University, with teaching experience in St. Andrews, Aberdeen, Brooklyn College, the University of California at Berkeley, and at Boston College, since 1962 Professor of Psychology at the University of Toronto, Professor Berlyne engages in research work in experimental psychology, particularly in the field of behavior theory. His special emphasis is on motivational problems and intellectual processes. The author of *Conflict, Arousal, and Curiosity* (1960), he has completed the manuscript of a second book entitled *Structure and Direction in Thinking*. The article here published deals with ten years of symposia on Motivation Theory known as the Nebraska Symposium. Address: Department of Psychology, University of Toronto, Toronto, Ontario, Canada.

ROBERT BIERI, *Huminoids On Other Plants?* 452

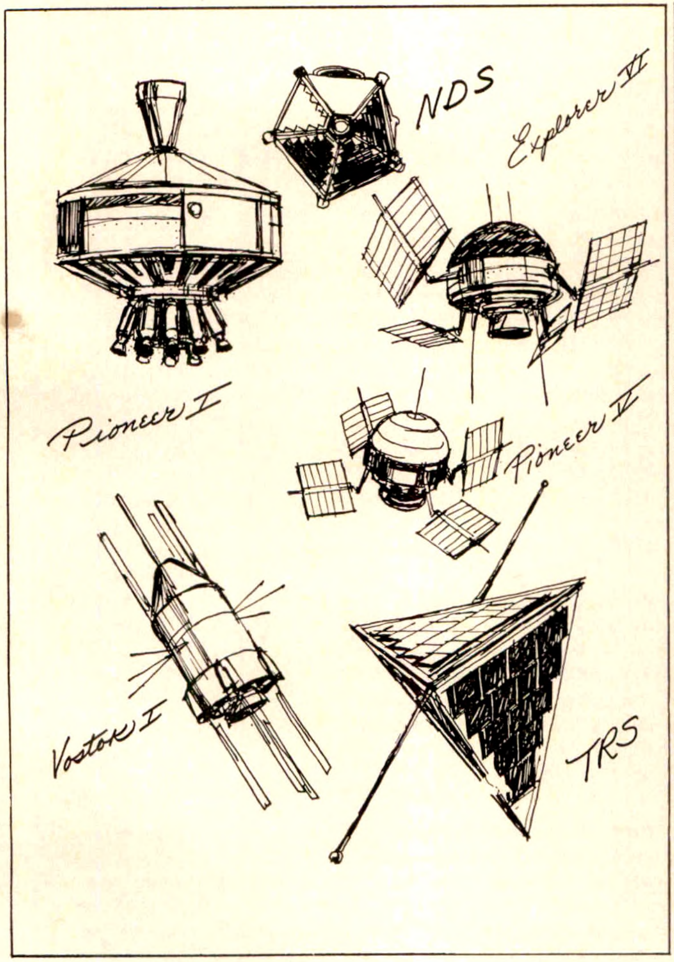
An Assistant Professor in Antioch College, where he graduated in 1949, received his Ph.D. degree in 1958 in Oceanography in the Graduate School at La Jolla. His research interests have been on marine zooplankton and, more recently, in forest ecology. The thesis of the present article is that given carbon dioxide and water the evolution of huminoids on other planets would be similar to that which has occurred on earth. Address: Department of Biology, Antioch College, Yellow Springs, Ohio.

J. W. LINNETT, *Valency and the Chemical Bond* 459

A Fellow of the Royal Society of London, who has just completed a leave of absence from Queen's College, Oxford at the University of California, Berkeley, here discusses the chemist's view on valency and the chemical bond. He presents a revised version of the famous octet and shared-pair electron theory of G. N. Lewis of fifty years ago. He indicates how the spins of the electrons suggest two quartets instead of octets and how the new views illuminate the concept of resonance structures first developed by Linus Pauling. Experience with freshman chemistry students on the Berkeley Campus suggests that the ideas evolved are welcomed by students and they lead also to quantitative formulations of molecular structures. His new book "The Electronic Structure of Molecules," an import by Wiley, N.Y., from Methuen, London, gives a wider examination of other molecules and of excited electronic states. Address: Queen's College, Oxford, England.

RICHARD B. ELLIS, *Dispersion Strengthening of Metals* 476

The Head of the Inorganic Materials Section at Southern Research Institute, Birmingham, Alabama, Dr. Ellis received his Ph.D. degree in analytical and physical chemistry at Vanderbilt University in 1940, where, as an undergraduate he had been elected to the Phi Beta Kappa Society. He has held research positions with Seagram's and with Corning Glass Works and was formerly a professor at the Universities of Florida and Miami. His research interests include molten salts, liquid metals, boron chemistry, ceramic fibers, and atomic fuel reactor systems, especially in the area of surface phenomena and solid-solid interfaces.



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Spacecraft above: Pioneer I, lunar probe, launched Oct. 1958; Explorer VI, scientific satellite, launched Aug. 1959; Pioneer V, interplanetary probe, launched March 1960; TRS, tetrahedral research satellite, launched 1962; NDS, nuclear detection satellite, launched 1963; Vostok I, manned Russian spacecraft, launched April 1961.

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A. J. EARDLEY, *Polar Rise and Equatorial Fall of Sea Level* 488

With the permission of the Editor of the *Journal of Geological Education,* we reproduce the Presidential Address to the National Association of Geology Teachers, published in Volume 12 of the Journal for March 1964. President Eardley graduated A.B. from the University of Utah in 1927 and earned his Ph.D. degree in Princeton. After nineteen years at the University of Michigan, he became Head of the Department of Geology at the University of Utah in 1949. Since 1954, he has been Dean of its College of Mines and Mineral Industries. A Sigma Xi National Lecturer in 1957, he is, at present, President of the American Geological Institute. His principal research interests are concerned with structure and evolution of North America and he is the author of several books. Address: College of Mines and Mineral Industries, University of Utah, Salt Lake City.

HUGH TAYLOR: *Perspectives—Academia and Industry—Their Mutual Influence* 498

For the William Procter Prize Address to RESA for 1964, the Editor-in-Chief of AMERICAN SCIENTIST reviews, in this Perspectives article, the academic and industrial origins of several sciences, the tools and techniques of modern research as contrasted with those available forty years ago, and scans the future.

HUGH H. SKILLING, *An Operational View* 388A

A professor of Electrical Engineering in Stanford University, interested in training young students who plan to teach science and engineering, is the author ofs ix books and numerous technical and research papers. One of his books, "Exploring Electricity," tells of the development of electrical science. Chairman of his department for over twenty years his principal research interests are in the domain of high voltage electricity and network theory. In this article he discusses our concepts of models visual, mathematical and conceptual.

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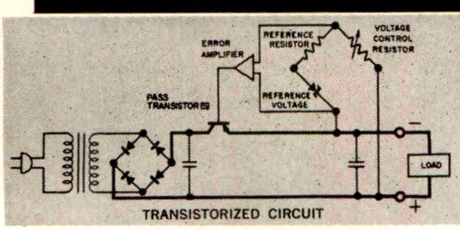

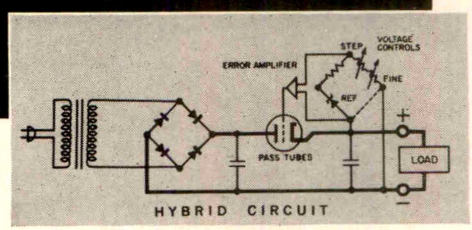

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NEWS AND VIEWS

By the Board of Editors and the Membership of the Sigma Xi-RESA Societies

ANNUAL CONVENTION OF THE SOCIETY OF THE SIGMA XI

In accordance with Article III, Section 1 of the Constitution notice is hereby given that the 65th Annual Convention of The Society of the Sigma Xi will be held on December 29, 1964, at 9:00 A.M. in association with the AAAS meetings at the Queen Elizabeth Hotel, Montreal, Canada.

Article III, Section 2(a) of the Constitution provides: Each Sigma Xi chapter is entitled to be represented at the Convention by not more than three delegates. Appointment of each chapter delegate shall be certified by the president or secretary of the chapter represented. Delegates representing a chapter shall be chosen from its chapter members, except that a chapter unable to appoint any or all of its chapter members may appoint from its affiliate members or from chapter members of other chapters. Each chapter represented at the Convention shall be entitled to one vote on all issues before the Convention.

Article III, Section 2(b) of the Constitution provides: Each Sigma Xi club is entitled to be represented at the Convention by not more than three delegates. Appointments of each club delegate shall be certified by the president or secretary of the club represented. Delegates representing a club shall be chosen from its members except that a club unable to appoint any or all of its delegates from its members may appoint as delegate any active member of the Society. Each club represented at the Convention shall be entitled to one vote on issues directly affecting the clubs. All club delegates shall have the privilege of the floor to discuss any subject before the Convention.

It is expected that the chapters and clubs will make every effort to be represented at the Convention. The names of the delegates should be sent to the office of Sigma Xi at once or not later than December 21, 1964.

For further information regarding procedure for the Convention, please see Section 3.131 of the Manual of Procedure.

Convention Program

December 29, 1964
65th Annual Convention
Queen Elizabeth Hotel—Bersimis Room

8:00— 9:00 A.M.	—Registration
9:00— Noon	—First Session
Noon—1:30 P.M.	—Luncheon*
1:30— 3:30 P.M.	—Second Session

* Luncheon tickets will be available in the Queen Elizabeth Hotel, Bersimis Room, from 8:00 A.M. until 9:00 A.M. on December 29 and delegates for whom reservations have been made may pay fee and pick up tickets at that time. Total capacity is limited and therefore it is advisable to secure a reservation.

Joint Sigma Xi-Phi Beta Kappa Address
Queen Elizabeth Hotel
Grand Ballroom 8:30 P.M.
Speaker: Dr. René Dubos
Address: "Humanistic Biology"

AGENDA
65th ANNUAL CONVENTION

December 29, 1964

Queen Elizabeth Hotel, Montreal, Canada

1. Appointment by the President of three members to serve as a Committee on Credentials
2. Report of the Committee on Credentials

3. Approval of Proceedings of the 64th Annual Convention
4. Report of the President
5. Report of the Executive Secretary
6. Report of the Treasurer
7. Report and Recommendations from the Executive Committee:
   A. National Assessments and Fees for 1965
   B. Petitions for Chapter Status from:*
      1. Arizona State University
      2. Colorado School of Mines
      3. Hamilton College
      4. Northeastern University
      5. South Dakota School of Mines
      6. Wake Forest College
   C. Proposed Revision of the Constitution and Bylaws based on recommendation of the Committee on Long-Range Planning
8. Report and Recommendations from the Committee on Criteria for, and Election to, Membership in the Society
   A. Proposed Revision of the Constitution and Bylaws
9. Report of Editor-in-Chief of AMERICAN SCIENTIST
10. Report of Editor of SCIENCE IN PROGRESS
11. Reports of other Committees:
    A. Membership-at-Large
    B. National Lectureships
    C. Grants-in-Aid of Research
12. Report of Nominating Committee and Election of Officers
13. General Business
14. Adjournment

Suggested candidates for the officers may be sent to any member of the Nominating Committee:

Dr. Alexander Hollaender, Oak Ridge National Laboratory

Dr. H. Richard Crane, University of Michigan

Dr. Herbert E. Longenecker, Tulane University

Dr. George H. Boyd, University of Georgia

Dr. Gerald J. Lieberman, Stanford University, Chairman.

REVISIONS TO THE CONSTITUTION AND BYLAWS

The 65th Annual Convention of The Society of the Sigma Xi will be asked to consider a number of revisions to the Constitution and Bylaws. In general, these revisions reflect four proposed actions:

1. To improve procedures for election to membership with more clearly defined distinctions between associate and full membership as recommended by the Committee on Criteria for, and Election to, Membership.
2. To authorize a revised system for the collection of dues from members whereby national dues are billed nationally on a direct individual basis and local dues are collected either locally or nationally at the option of the local chapter or club.
3. To establish a Standing Committee on Publications.
4. To continue the immediate Past President as a member of the Executive Committee for a two-year period.

The first of these proposals will be presented to the Convention by the Committee on Criteria for, and Election to, Membership, with the recommendation for approval by the Executive Committee. The details of this proposal are presented in the report of the Committee on Criteria for Election to Membership, see page 369A. The remaining proposals are recommendations of the Executive Committee resulting from studies by the Committee on Long-Range Planning. The details of these proposals are as follows:

Basis of Proposals

1. Direct National Billing of Members: It has been 10 years since National Headquarters introduced the present system of maintaining the membership records and collecting the national assessments. In this decade the size of the Society has essentially doubled from 44,000 to 85,000 members. The 117 chapters of 1953 have become 145, the 60 clubs-133, the 5000 Members-at-

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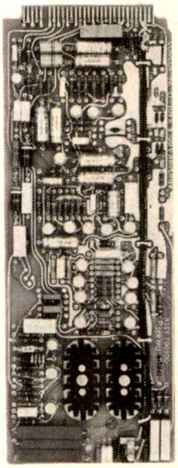

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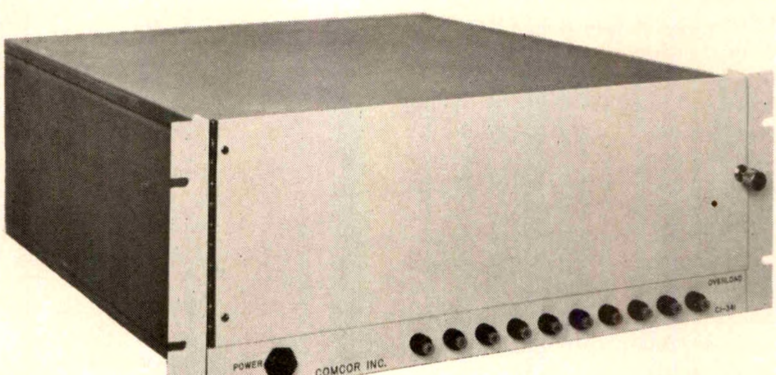

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Large-25,000, and, of course, all other activities have multiplied accordingly. The work load at National Headquarters, however, has increased by some geometric proportion.

In 1963, the Executive Committee engaged an outstanding firm of management consultants to study certain aspects of the Society's current and proposed operations. Included in the report submitted by the consultants were the following statements:

Recommendations for Creating a Sound Administrative Structure

"Sigma Xi will always have to tolerate some of the limitations that now exist locally. It will always be difficult to get chapter officers to serve for a long period of time, and these positions will always be filled by busy people whose major interests lie elsewhere.

"Therefore, to strengthen local administration, local needs should be provided for at a level in the Society where both time and continuity of personnel can be made available economically.

"We recommend that you strengthen National Headquarters to improve administrative support to the chapters and clubs, and to establish a closer bond between National Headquarters and individual chapter members. To put this recommendation into effect Sigma Xi should:

(*1*) "*Provide Systems Information to the Chapters and Clubs:* National Headquarters should develop and circulate to the chapters a complete set of systems and procedures for their record-keeping and administrative needs.

(*2*) "*Provide Central Dues Collection and Mailing Services:* Since up-to-date address lists must be maintained for AMERICAN SCIENTIST, New Haven is already well equipped to bill chapter members directly for dues and to rebate local dues to individual chapters. This service already is performed for the Members-at-Large, and central dues collection is successfully carried out by other major societies, such as the American Chemical Society.

(*3*) "*Establish Direct Communications with Chapter Members:* There now almost no direct contact betwe National Headquarters and the in vidual chapter member. We reco mend that National Headquarters ke members posted on chapter activit and national programs throughout t country. Establishing this line of co munication would give National Hea quarters the opportunity to reemph size the key elements of the Societ philosophy continually and to point the national and international aspe of the Society's activities, which n tend to be overlooked locally.

"The primary vehicle for this co munication should be a regular 'Ne of the Society' section in AMERIC SCIENTIST. In addition, communic tions might be enclosed within the du mailings and occasional special ma ings—although the latter would expensive and should be strict limited in number."

The Committee on Long-Range Pla ning appointed by President Rossi to study this report requested the Exec tive Secretary to prepare a plan for t direct billing of the membership whereb the following objectives would realized:

1. Improved Headquarters oper tions,
2. Improved image of the Nation Society, and
3. Improved relations with individu members of the Society as we as with the chapters and clubs.

The proposal was first submitted i principle by the Executive Secreta to the Long-Range Planning Committe on 10 March 1964. Following approv in principle—a detailed formal pr posal (including computer program wit time and cost estimates) was furnishe the Committee on 15 May. This pla in summary is as follows:

PROPOSED PLAN

Principal Features:

1. Change the period covered by Na tional Assessment from calenda year to academic year. This would b consistent with essentially 100% of the chapters and clubs and woul thus eliminate much of the presen

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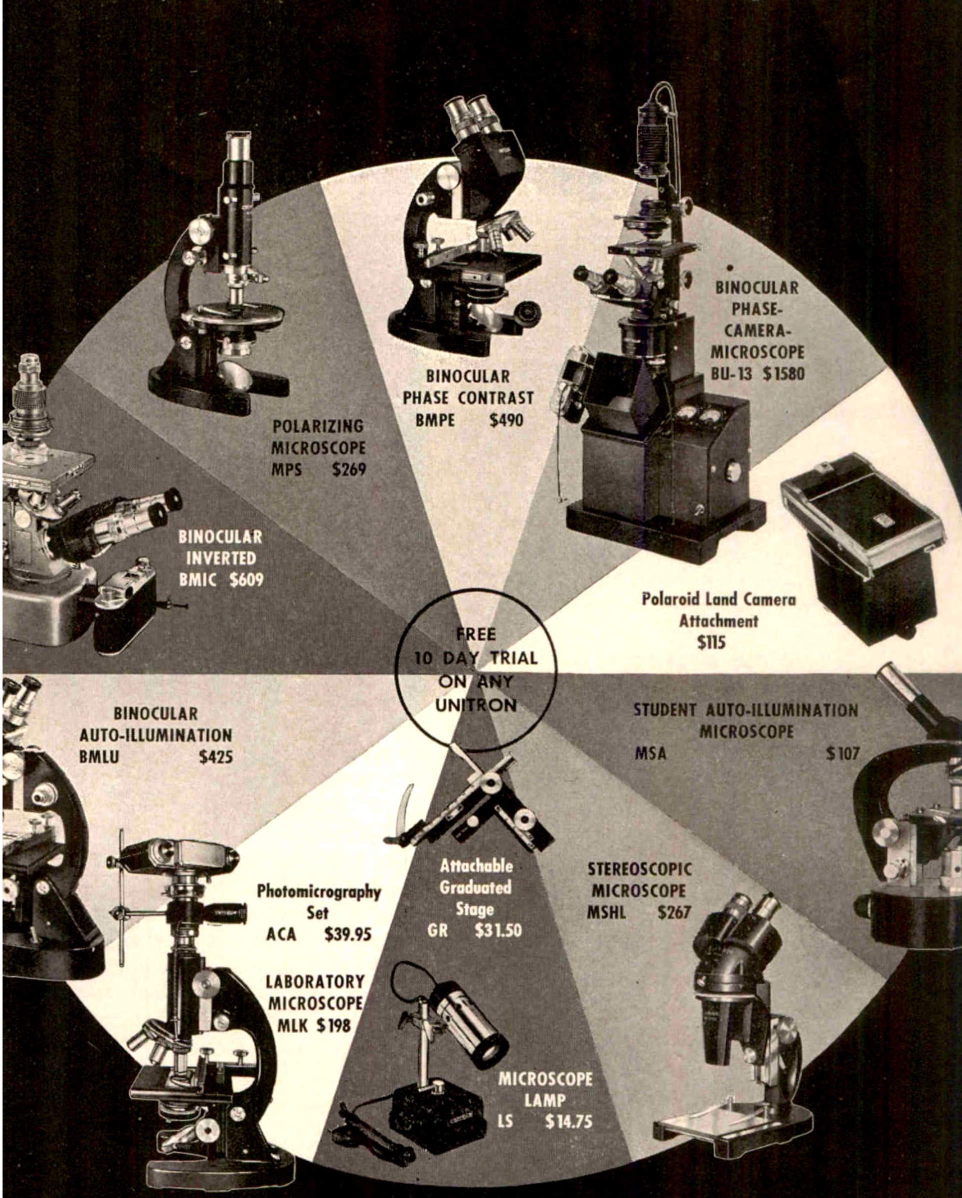

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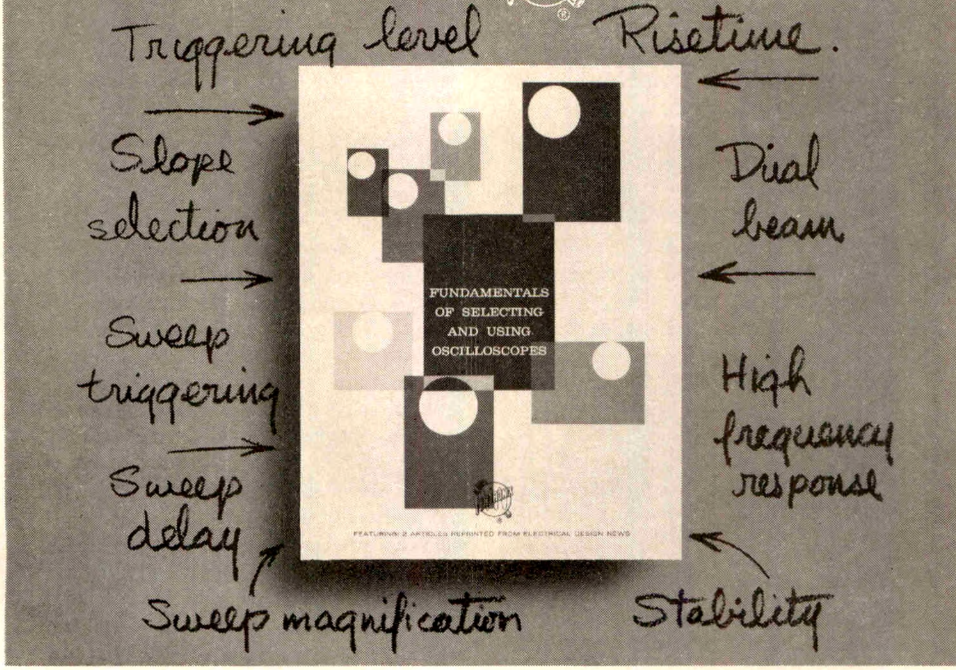
confusion and dissatisfaction. The fiscal year of the Society would remain unchanged.

2. Encourage chapters to hold only Spring initiations.
3. Increase initiation fees by at least $1.00 to provide for an additional one-half year subscription to AMERICAN SCIENTIST. This would insure receipt of magazine for one full year after name has been added to rolls of the Society. (New initiate would continue to be active for the academic year following that in which he was initiated.)
4. Convert entire record-keeping system and mailing procedure to a magnetic tape operation for speed, flexibility, and economy of operation.
5. Introduce a new universal membership bill with following features:
   A. A detachable membership card for academic year of billing.
   B. A one-piece form which will fold into a postage paid return envelope with space for:
      (1) Message to and from membership
      (2) National dues entry
      (3) Chapter/club local dues entry—or notation "Pay locally"
      Note: This is an option feature to be used in accordance with wishes of chapter or club.
      (4) Contribution to Grants-in-Aid of Research
      (5) Total
      (6) Name
      (7) Address
      (8) Identification number (chapter, club, etc.).
6. Bill membership as follows:
   A. New initiates.
      (1) Send a bill with the National Assessment entry marked "Paid" to most all intiates on June 1 following initiation—establishing immediate contact. Possibly include letter from National President.
      (2) Continue to send "Paid" bills as late initiate data cards are received after June 1.
   B. Active membership of institutional chapters and clubs.
      (1) Send bills to all chapter and club members on May 15.
      (2) "Charge" variable local dues of current chapter or club *if requested to do so by that chapter or club.*
      (3) Send record of complete billing to each chapter or club concerned on May 15.
      (4) Repeat billing of unpaid membership on June 15, August 1, September 15—sending record to each chapter and club concerned.
   C. Chapter-at-Large Membership.
      (1) First bill to Membership-at-Large would have to roll the dues forward or backward—if effective date becomes July 1 rather than January 1—to ease transfers.
7. Allocate individuals and fees paid to chapters and clubs as requested by members.
8. Send current membership list with addresses to chapters and clubs on September 15, with local dues collected, and repeat this operation October 15, November 15. Thereafter, additions reported by individual notice, not machine run.
   A. List would contain:
      (1) Names in alphabetical order.
      (2) Addresses.
      (3) Complete data on each member. (Associates and Full would be separate or clearly indicated.)

Advantages:

1. Places member on a direct individual basis with National Headquarters:
   A. Eliminates many inaccuracies and delays since National Office need not depend on chapter or club to transmit fees.
   B. Builds image of *National Society*.
   C. Permits National Office to respond to a complaint immediately without the present check through chapter or club.

SOME OSCILLOSCOPE TERMS AND TECHNIQUES EXPLAINED IN THIS FREE TEKTRONIX BOOKLET



Choosing the *right* oscilloscope to help solve a measurement problem is sometimes confusing. The choice seems so large. For example, Tektronix offers over fifty different types.

The choice of an oscilloscope narrows considerably, however, once the application is known. Determining the *type* which best suits the application then becomes a matter of understanding the various features of the oscilloscope.

To help you better understand oscilloscope features, Tektronix offers you a free booklet. The booklet, FUNDAMENTALS OF SELECTING AND USING OSCILLOSCOPES, can be an invaluable aid in furthering your knowledge of oscilloscopes and in learning more about how these precision tools might help you in your studies of changing phenomena. Also, in addition to explaining oscilloscope features, this informative 16-page booklet designates differences in oscilloscope types and describes factors affecting validity of waveform displays.

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2. Saves time of chapter and club non-paid officers.
3. Saves chapters and clubs postage, etc. when local dues are collected.
4. Eliminates wait of 2 months by National Headquarters for edited list.

Actually, the present method can result in a member paying his dues in September to a chapter or club and not becoming active until May of the next year.

Files constantly corrected as payments are received. Fewer missed issues of AMERICAN SCIENTIST will reduce back mailing required.

5. Catches transfers earlier and automatically.
6. Saves on AMERICAN SCIENTIST—no really free issues.

Initiation fee provides for six issues including March and June of year of election.

7. Maintains constant contact with membership permitting important communications to be sent directly.
8. Affords opportunity for every member to contribute to Grants-in-Aid of Research.
9. Evens out work load on office of the Treasurer.
10. Affords greater safety to records. (Duplicate copies can be stored in fireproof vault—present voluminous files cannot.)

Following a month's study of the plan the Committee on Long-Range Planning met on June 11 and after full consideration of comments from chapters and clubs, *VOTED* that:

The Long-Range Planning Committee recommend to the Executive Committee that, at its meeting on October 16 (later changed to October 7), appropriate proposals be made by the Executive Committee to the 65th Annual Convention on December 29 so that:

A. National dues may be billed nationally on a direct individual basis in accordance with the detailed plan prepared by the Executive Secretary.

B. Local dues may, at the option of the local chapter or club, be collected either locally or nationally.

C. The National Society will absorb inequities arising from transfers of membership from one local chapter or club to another, so that a member in good standing be in no less a position after moving. (This will cover variations in local dues, with some chapters or clubs with higher dues receiving appropriate payments.)

2. Committee on Publications: Article IV Section 2 of the Constitution provides that "Unless the Executive Committee shall make other arrangements, the Executive Secretary shall edit the Society publications." In 1954, upon the recommendation of an Ad Hoc Committee on Publications, the Executive Committee appointed an Editor-in-Chief and Board of Editors for AMERICAN SCIENTIST. The Editor-in-chief of AMERICAN SCIENTIST also served as the Editor of *Science in Progress* until 1960 when a separate Editor was appointed for that publication.

Within the past decade, not only has the size of AMERICAN SCIENTIST increased by approximately 50%—the circulation has more than doubled. With this increase in the magnitude of the operation, it seemed advisable to the Committee on Long-Range Planning that the Executive Secretary and the Executive Committee could benefit from the support of a Standing Committee on Publications which would serve in an advisory capacity on matters pertaining to publications of the Society other than editorial.

The Committee on Long-Range Planning at its meeting on June 11 unanimously *VOTED* that:

The Long-Range Planning Committee recommend to the Executive Committee that an Advisory Committee on Publications be appointed in a manner similar to other committees (Lectureships, Grants-in-Aid) and that it consist of five members (four Sigma Xi and one RESA) and that its scope be established by the Executive Committee. Also, that the Editor-in-chief and the Business Manager of AMERICAN SCIENTIST be ex-officiis Members and that the Editor-in-chief be Chairman of this Committee.

3. Past President: Since the last major revision of the Constitution and Bylaws in 1956, members of the Executive Committee have suggested that the immediate Past President should continue as an Ex-officio member of the Executive Committee. For obvious reasons each President, in turn, has been reluctant to have this proposal presented when he would be personally concerned with the action. President Rossini requested that the recommendation to be made to this year's Convention become effective January 1, 1967. The Executive Committee, however, voted unanimously not to accept Dr. Rossini's proposed timing but rather to have the provision for representation on the Executive Committee by the immediate Past President to become effective immediately if approved by the Convention.

REPORT OF THE COMMITTEE ON CRITERIA FOR, AND ELECTION TO, MEMBERSHIP IN THE SOCIETY OF THE SIGMA XI

The Committee on Criteria for, and Election to, Membership in The Society of the Sigma Xi was appointed by President Rossini as a Committee of the Convention of the Society, by vote of the delegates in December 1962. This Committee was given the dual assignment of (1) reporting to the Convention on specific actions which would improve the position of the Membership-at-Large in the Society; and (2) reporting to the Convention on the Criteria which might be used for determining eligibility for election to, and membership in, the Society.

The first of these assignments was completed in October 1963, and the recommendations from the Committee were accepted by the Convention at its meeting in December of that year. Since that time the Committee has been concerned with identifying and defining the criteria which might be used for election to associate membership and (full) membership in the Society. As our discussion progressed, it became evident that the diversity of the criteria now allowed in the National Constitution and Bylaws made any approach to standardization of criteria for election impossible: accordingly, we have felt that changes should be made in the National Constitution and Bylaws to reduce to some extent this diversity.

As a basis for its deliberations, the Committee on Criteria studied copies of constitutions, bylaws, nomination forms, and other documents submitted by 121 of the chapters in the Society. From a survey of this material, it became clear that much diversity existed also in the procedures used by chapters in the election of Associate Members and (full) Members; some chapters use no nomination form, the nominations being made by letter or even orally to the membership committee; other chapters use some type of form, usually mimeographed, requesting varying amounts of information about the nominee. The Committee on Criteria is convinced that, quite apart from the changes which are being suggested in the National Constitution and Bylaws, a standard form of nomination blank prepared under the direction of the Executive Committee of the Society, could be used to good advantage by all chapters in the election procedures of both Associate and (full) Members. These forms would be distributed without charge to the chapters. One page of this form would contain excerpts from the National Constitution and Bylaws regarding the criteria for election. The Committee on Criteria has accordingly recommended to the Executive Committee of the Society that such forms be prepared and provided for the use of chapters. Since there is no statement in the National Constitution or Bylaws regarding the form on which nominations are to be made, no changes in the Constitution or Bylaws are necessary. The National Executive Committee has already accepted this recommendation and has authorized the Executive Secretary's office to prepare the nomination form. Printing and distribution of these forms will be delayed until action is taken by the Convention in December 1964, on the changes recommended by the Committee on Criteria, as given later.

The Committee on Criteria submits

the following recommendations to the Convention:

First: *That election to membership in The Society of the Sigma Xi, either to associate membership or (full) membership, be based only on research aptitude or research achievement and not upon academic standing in class, formal course grades or grade points or upon an analysis of the personal character of the nominee.*

At the present time the National Constitution and Bylaws state that eligibility for election to associate membership requires marked excellence in studies in at least one field of pure or applied science. Many chapters have extended this requirement of standing in class or course grades or grade points to the eligibility of graduate students. However, since The Society of the Sigma Xi is concerned with the recognition and the encouragement of research in science, it is the conviction of the members of the Committee on Criteria that research aptitude and achievement only should be used as criteria. In many cases, of course, there is direct relationship between high grades and research achievement but there are also many instances of exceptional research achievement without the high academic record in courses. In accordance with this proposal, the Committee on Criteria recommends the deletion from the Constitution of the reference to "excellence in studies" as a requirement for election to associate membership. (See Appendix I to this report.)

Second: *That election to associate membership be restricted to undergraduate students (at the time of election), including graduating seniors, who have shown marked aptitude for research. The aptitude is to be demonstrated by independent investigation ordinarily resulting in a written report (such as term paper, thesis, etc.), a copy of which must be made available to the local Committee on Admissions, if the members of the Committee so require.*

In explanation of this recommendation the Committee calls attention to the following points:

An analysis of letters and other documents received from the chapters of the Society in connection with the activities of the Committee on Criteria shows that it is almost always the graduate student group which presents the problems in election. Ordinarily, only a few undergraduates are elected and these are clearly unusual students. Likewise, the members of the instructional and research staff present little difficulty; their selection as officers of a university is usually sufficient indication of their research achievement. The graduate students are the cause of uncertainty to a large degree because they may be taken into the Society, under the present terms of the National Constitution, either as Associate Members or as (full) Members. There is at present, therefore, the double problem of determining the minimum standards for their election as (full) Members. The Committee on Criteria is of the opinion that problems of their election will be less extensive and acute if associate membership is restricted to undergraduates (that is, undergraduates at the time of their election). This change would also bring into sharp focus the real meaning of Associate Members: undergraduates who have shown marked aptitude for research on the basis of some research work actually accomplished. In addition, it emphasizes the responsibility of the chapters regarding Associate Members who become graduate students—the obligation to observe the progress of such individuals and to promote them to (full) Members when they have demonstrated the required research achievement. The Committee on Criteria is accordingly recommending that the pertinent article and section of the Constitution be changed so as to restrict the election of Associate Members to undergraduates. (See Appendix I to this report.)

Third: *That election or promotion to (full) membership in the Society be based on noteworthy achievement in research, demonstrated by publications, written reports, thesis, or dissertation, copies of which must be made available to the local Committee on Admissions, if the members of the committee so require. The local committees have the responsibility of setting their own standards within*

the framework of this general pattern.

The following is an explanation of the Committee's reasons for this proposal. The most controversial question with which the chapters have had to deal in connection with election of graduate students to (full) membership involves the significance of the students' meeting the requirements for the Ph.D. degree, in the case of those students who are completing their final year as graduate students. Some members of the Society believe that election to (full) membership in Sigma Xi should stand for the competititve best in research, which would not, of course, include every individual receiving a Ph.D. degree in the natural sciences. Others believe that the meeting of the requirements of the degree of Ph.D. in a natural science should in itself be sufficient to indicate research achievement. In considering this question, the Committee on Criteria has been increasingly impressed by the following statement included in Article V of the National Bylaws, regarding the conditions essential for election of (full) Members: "Evidence of suitability for election to Sigma Xi should comprise all those accomplishments expected of companions in zealous research that would normally be recognized by the independent award of degrees, by publication of research, or by other kinds of certification. However, membership in Sigma Xi is neither linked to the possession of any degree nor contingent upon belonging to some other organization." We wish to reiterate the assertion here that *membership is not linked to the possession of any degree.* It is our conviction, therefore, that the possession of the Ph.D. (including the completion of the requirements for this degree) does not by itself meet the conditions necessary for election, although, of course, *the research done for the degree requirements would form part of the evidence to be considered.* Some objection may be raised that a student completing his last year for the Ph.D. degree might not have his thesis in such a condition as to be submitted as evidence at the time when the election of members takes place. It is for this reason that the clause "if the members of the committee so require" is included in our proposal; much depends upon the local circumstances, the time when elections are held, etc. It is also stated in our proposal that the local committees have the responsibility of setting their own standards within the framework of the general pattern. What the Committee is trying to emphasize is that there should be no such process as "rubber-stamping" of election of members into the Society and that local committees have their own responsibility and obligation of satisfying themselves of the noteworthy achievement accomplished by the nominee.

In connection with the requirements for election to (full) membership in the Society, the Committee believes that the policy of some chapters of setting two different standards for election to (full) membership, one for graduate students and another for staff members, is unsatisfactory. This policy has resulted in peculiar situations, e.g, an assistant professor not being eligible for membership in a chapter, although his graduate students are because of lower standards for the student category. The Committee on Criteria feels that there should not be double standards of this type and is proposing that the section of the Bylaws pertaining to (full) membership carry a statement that criteria for election of staff members are the same as those for graduate students. (See Appendix I of this report.)

There is one additional change in the general area of membership which the Committee on Criteria is recommending to the Convention, although criteria as such are not specifically involved, on the grounds that the present arrangements lead to confusion and misunderstanding. The National Constitution (Article VII, Section 2a), now specifies that, among others (professors, instructors, etc.), *graduates* of an institution in which a chapter is located may be elected to membership. Originally there was, in fact, a category of membership designated as *alumni members* for such individuals, but this was deleted from the Constitution several years ago and

the graduates provided for in Section 2a, as mentioned above. At that time, Section 2c of Article VII was not in existence and it seemed advisable to make such provisions for graduates who would otherwise not be eligible for election. More recently, however, Section 2c was added, this providing possibilities for membership for any investigator connected with a non-degree-granting institution, not having a chapter; this broad statement eliminates the need for provision specifically for electing graduates. Many chapters have continued to recognize *alumni members* in their Chapter Bylaws, though this category of membership was deleted from the National Constitution years ago. Furthermore, some chapters have set up complicated limitations on the eligibility of graduates, such as the number of years which have passed since attending university, etc. The graduate category also complicates the election of investigators at a non-degree-granting institution not having a chapter as now indicated in Section 2a of Article VII of the Constitution. The Committee feels that this graduate provision is no longer needed and is proposing its elimination from the Constitution. The Committee wishes to emphasize, however, that this change is not strictly a part of the recommendations having to do with the criteria for election.

Accompanying this report in Appendix I is a list of the changes in the National Constitution and Bylaws proposed by the Committee on Criteria. Appendix II is a list of the corresponding sections and articles as they now exist. As will be noted, the Committee has taken advantage of this opportunity to revise to some extent the wording of the Sections of both the Constitution and the Bylaws pertaining to membership, the wording being changed in such a way as to give greater emphasis to the varying circumstances under which elections may be made. We have also revised the order of some of the Sections, by putting the associate membership category before the (full) membership category; thus, Section 2 of Article VII deals with associate membership and Section 3 deals with (full) membership or promotion from associate membership to (full) membership.

Please note that in the proposed revision of the Articles of the Constitution, following a general statement of eligibility, reference is made to Articles in the Bylaws which define terms or amplify the meanings of the Constitution. For example, in the revised Article VII, Section 2 of the Constitution, the statement is made that: "The following are eligible for election to associate membership as provided in Article VIII; in an institution having a chapter, any undergraduate who has shown marked aptitude for research in the field of pure or applied science (see Bylaws Article V, Section 2)." In the Section of the Bylaws referred to, the nature of the evidence which may be used to determine research aptitude is defined.

Section 3 of Article VII of the Constitution has been revised in such a way as to make the differences between the three portions of the Section clear; the first (a), deals with institutions having a chapter; the second (b) with a degree-granting institution not having a chapter; and (c) with a non-degree-granting institution not having a chapter.

Committee on Criteria for, and Election to, Membership
Marsh W. White, A. C. Leopold, Harold G. Cassidy, James H. Marks, Frank C. Croxton, F. M. Carpenter, Chairman

Appendix I of the Report of the Committee on Criteria for, and Election to Membership in The Society of the Sigma Xi states the proposed changes in the National Constitution and Bylaws. Appendix II records the corresponding sections of the Constitution and Bylaws of the Society as they now exist.

The proposed revisions of the Constitution and Bylaws thus involved have been added to the revisions proposed by the Committee on Long-Range Planning and are presented, as a whole, in the next pages to show both present and proposed wording.

PRESENT WORDING

ARTICLE IV

Section 1. The officers of the Society shall be a President, a President-elect, an Executive Secretary, and a Treasurer. The standing committees of the Society shall include Executive, Membership-at-Large, Grants-in-Aid of Research, National Lectureships, Budget, and Finance.

Section 3. (a) The Executive Committee shall consist of the following: The President, President-elect, Executive Secretary, Treasurer, eight members elected by Annual Conventions, the Chairman of the Committee on Membership-at-Large, the Chairman of the Committee on Grants-in-Aid of Research, the Director of National Lectureships, and the Editor-in-Chief of *American Scientist* if other than the Executive Secretary as provided by Section 2 of this Article.

ARTICLE VII

Section 3. The following and no others are eligible for election to associate membership by any chapter as provided in Article VIII: Any person who is currently or was during the preceding year a student in the institution in which the chapter is located and has completed at least three years of work toward an undergraduate degree and has shown marked excellence in his studies in at least one field of pure or applied science, as described in Article I, Section 3, and given evidence of an aptitude for research in one of these fields.

Section 2. The following and no others are eligible for election to membership or promotion from associate membership to membership by any chapter, as provided in Article VIII.

(a) Any professor, instructor, student, graduate, or other member of the instructional or research staff of the institution in which the chapter is located, who has shown noteworthy achieve-

PROPOSED WORDING

ARTICLE IV

Section 1. The officers of the Society shall be a President, a President-elect, an Executive Secretary, and a Treasurer. The standing committees of the Society shall include Executive, Membership-at-Large, Grants-in-Aid of Research, National Lectureships, Budget, Finance, and Publications.

Section 3. (a) The Executive Committee shall consist of the following: The President, President-elect, Executive Secretary, Treasurer, eight members elected by Annual Conventions, the immediate Past President, the Chairman of the Committee on Membership-at-Large, the Chairman of the Committee on Grants-in-Aid of Research, the Director of National Lectureships, and the Editor-in-Chief of AMERICAN SCIENTIST if other than the Executive Secretary as provided by Section 2 of this Article.

ARTICLE VII

Section 2. The following are eligible for election to associate membership as provided in Article VIII: In an institution having a chapter, any undergraduate who has shown marked aptitude for research in the field of pure or applied science (see Bylaws, Article V, Section 2).

Section 3. The following are eligible for election to membership or promotion from associate membership to membership by any chapter, as provided in Article VIII (see Bylaws, Article V, Section 3):

(a) In an institution having a chapter, any graduate student or member of the institution or of the research staff who has shown noteworthy achievement as an original investigator in some field of pure or applied science, as described in Article I, Section 3.

(b) In a degree-granting institution not having a chapter, any member of the instructional or research staff who has shown noteworthy achievement as an

(*Continued on page 377A*)

PRESENT WORDING

(*Continued from page 376A*)

ment as an original investigator in some branch of pure or applied science, as described in Article I, Section 3.

(b) Any professor, instructor, or other investigator connected with a degree-granting institution not having a chapter, who would otherwise be eligible for membership on the conditions stated in Section 2 (a) of this Article.

(c) Any investigator connected with a non-degree-granting institution not having a chapter, who would otherwise be eligible for membership on the conditions stated in Section 2 (a) of this Article, when the nomination by the chapter's Committee on Admissions has been approved by a three-fourths vote of the (National) Executive Committee. In those cases where the individual being nominated holds a research doctor's degree from an institution now having a chapter of Sigma Xi it should be clearly indicated that the election is being proposed subsequent to having extended the privilege of election to the chapter at the original institution.

Section 4. (a) A Member or Associate Member of the Society upon presenting satisfactory credentials showing election to membership or associate membership in the Society, and paying chapter dues, is entitled to appropriate membership, in any chapter depending upon his relationship to the institution in which the chapter is located.

(b) A Member or Associate Member of the Society upon presenting satisfactory credentials showing election to membership or associate membership in the Society, and paying dues, is entitled to membership in any club.

ARTICLE VIII

Section 2. It shall be the duty of the chapter or its Committee on Admissions

PROPOSED WORDING

original investigator in some field of pure or applied science, as described in Article I, Section 3.

(c) In a non-degree-granting institution not having a chapter, any investigator who has shown noteworthy achievement as an original investigator in some field of pure or applied science, as described in Article I, Section 3. Nominations by a chapter's Committee on Admissions must be approved by a three-fourths vote of the (National) Executive Committee.

Section 4. (a) A Member or Associate Member of the Society upon presenting satisfactory credentials showing election to membership or associate membership in the Society, and paying national and local chapter dues, is entitled to appropriate membership in any chapter depending upon his relationship to the institution in which the chapter is located.

(b) A Member or Associate Member of the Society upon presenting satisfactory credentials showing election to membership or associate membership in the Society, and paying national and local club dues, is entitled to membership in any club.

ARTICLE VIII

Section 2. It shall be the duty of the chapter or its Committee on Admissions

(*Continued on page 378A*)

PRESENT WORDING

(*Continued from page 377A*)

to examine annually the records of all Associate Members currently active with the chapter and of those previously elected by the chapter and to present to the chapter for promotion to membership such Associate Members as have shown noteworthy achievement as original investigators in some branch of pure or applied science. Also any Associate Member, as a basis of consideration for promotion to membership, may submit evidence of research accomplished to the secretary of the chapter which elected him or to the secretary of the chapter with which he is currently active.

ARTICLE XI

Section 1. The operating expenses of the Society shall be met to the extent necessary by such assessments on the chapters and clubs, as may be recommended by the Executive Committee and authorized by plurality vote of the chapters, clubs, and Chapter-at-Large represented at the Convention. The assessment of each chapter shall be based on a pro rata levy and computed on the basis of the number of Members and Associate Members on the official membership roll of the chapter, including chapter Members, chapter Associate Members, Affiliate Members and Affiliate Associate Members. The assessment on each club shall similarly be computed on the number of Members and Associate Members of the club.

Section 3. Members and Associate Members of a chapter or club shall pay to that chapter or club such annual dues as required by the bylaws of that chapter or club.

Section 4. Members and Associate Members active with the Chapter-at-Large shall contribute to the support of the National Organization in such ways as may be determined by the Convention on the recommendation of the Executive Committee.

PROPOSED WORDING

to examine annually the records of all Associate Members currently active with the chapter and to present to the chapter for promotion to membership such Associate Members as have shown noteworthy achievement as original investigators in some branch of pure or applied science. Also, any Associate Member, as a basis of consideration for promotion to membership, may submit evidence of research accomplished to the secretary of the chapter which elected him or to the secretary of the chapter with which he is currently active.

ARTICLE XI

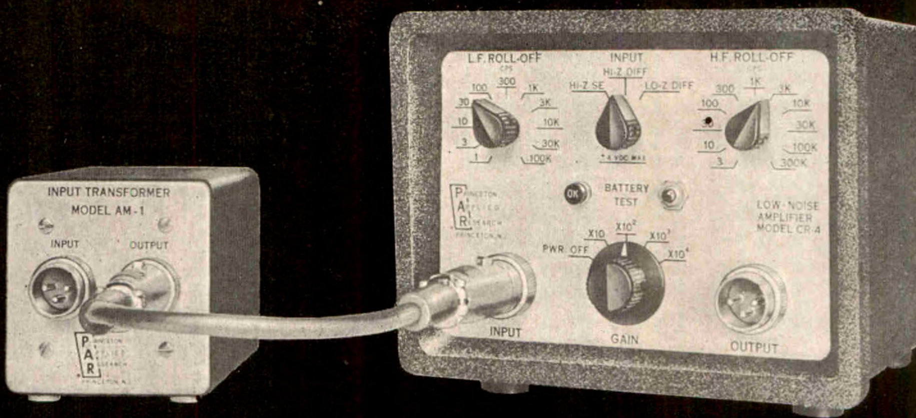
Section 1. (a) The operating expenses of the National Society shall be met to the extent necessary by such dues from the Members and Associate Members as may be recommended by the Executive Committee and authorized by plurality vote of the chapters, clubs, and Chapter-at-Large represented at the Convention.

(b) Dues shall be on an annual basis except for those Members and Associate Members, who, by the payment of a Life Membership fee, shall continue in good standing for life without further payment.

(c) The fee for Life Membership shall be voted by the Convention upon recommendation of the Executive Committee.

Section 3. Members and Associate Members of an institutional chapter or club shall pay to that chapter or club, either directly or through the National Treasurer, at the option of the chapter or club, such annual local dues as required by the bylaws of that chapter or club.

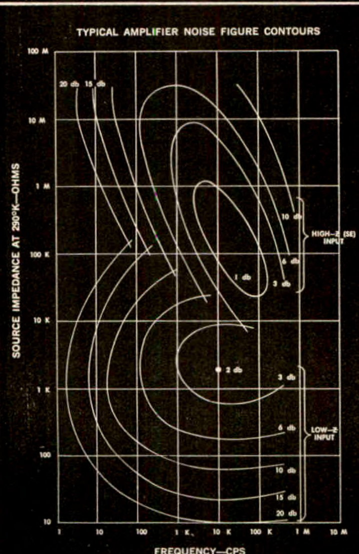
No comparable Section 4.



Low-Noise High-Gain Amplifier

The PAR Model CR-4 Low-Noise High-Gain Amplifier is designed for extremely small signal pre-amplifier applications and for use with PAR Lock-In Amplifiers to extend their low-level signal recovery capabilities. The unit features:

- differential or single-ended input
- selectable high (50M) or low (50K) input impedance
- 20 to 80 DB gain
- selectable bandpass 1 CPS to 300 KC
- long-life Hg or rechargeable NiCd battery pack
- completely transistorized
- easily panel mounted
- rugged printed circuit construction
- 6½" wide, 5" high, 8¾" deep
- price without batteries $575.00
- also available: several optional features such as impedance matching input transformer and fast-recovery Model CR-4A with selectable bandpass from 10 CPS to 300 KC



NOISE FIGURE CONTOURS

Experiments in general are performed at some definite source impedance level and over some definite frequency bandwidth. Noise referred to shorted input as frequently specified does not provide meaningful information for acutal experimental applications. It is instead essential to know the noise characteristics of an amplifier not only at the particular operating frequencies but also with the particular source impedances actually encountered. Noise figure contours are the loci of points of constant noise figure (F) plotted as a function of source impedance and operating frequency, and, therefore, provide this complete information. They indicate the degradation in signal-to-noise ratio obtained with the pre-amplifier at a particular frequency with a particular source impedance, input noise being the Johnson noise generated by that source impedance at 290 degrees Kelvin. The contours are obtained from the following equation:

$$F = 20 \log_{10} \frac{\text{total measured amplifier noise (including Johnson noise)}}{\text{calculated Johnson noise}} \text{ DB}$$

Write for Bulletin No. 117

P A R

PRINCETON
APPLIED RESEARCH CORP.
Dept. C
BOX 565, PRINCETON, N. J.

PRESENT WORDING

ARTICLE V

Section 3. Associate Membership. Marked excellence in studies as specified for eligibility for election to associate membership, in Article VII, Section 3 of the Constitution, should not require as high a level of attainment as the noteworthy achievement specified for full membership. Election to associate membership would normally apply to an undergraduate, or a graduate student, and would rest upon grades in courses and evidence of an aptitude for research.

Section 2. Full Membership. Noteworthy achievement as an original investigator in some branch of pure or applied science, specified as a criterion of eligibility for election to full membership, in Article VII, Section 2 (a) of the Constitution, must be judged on performance.

ARTICLE VI

No comparable Section 3.

PROPOSED WORDING

ARTICLE V

Section 2. Associate Membership. Research aptitude specified for election to associate membership in Article VII, Section 2, of the Constitution is to be demonstrated by independent investigation ordinarily resulting in a written report, which must be made available, if requested, to the local Committee on Admissions as a basis for selection.

Section 3. Full Membership. Noteworthy achievement in research specified for full membership in Article VII, Section 3, of the Constitution must be based on publications, written reports, a thesis, or a dissertation, which must be made available, if requested, to the local Committee on Admissions as a basis for selection. The possession of a doctorate or the meeting of requirements for such a degree by the nominee, does not, by itself, satisfy the requirements for election. Criteria for election of staff members are to be the same as those for graduate students.

Section 4. Statement of Responsibility. It is the responsibility of the local Admissions Committee to select for membership those individuals whose research aptitude or achievement deserves special recognition. Membership in The Society of the Sigma Xi is an honor and requires discriminating action by the local Admissions Committee.

ARTICLE VI

Section 3. (a) The Committee on Publications shall consist of the Editor-in-Chief of AMERICAN SCIENTIST as Chairman, together with four other members appointed annually by the President upon recommendation of the Chairman.

(b) It shall be the duty of the Committee on Publications to advise the Executive Committee on matters other than editorial which concern publications of the Society.

Solution by Simulation

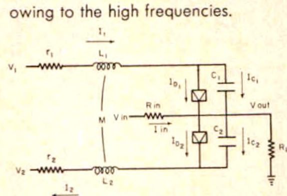
TUNNEL DIODE SWITCHING SIMULATION—BY ANALOG COMPUTER

Circuit and component designers find analog simulation a rapid and highly creative alternative to costly and time-consuming breadboarding. The analog computer, containing a collection of flexible computing components which can be "patched" together, enables you to build a model of the system or circuit and simulate its dynamic performance.

The ease of component interconnection on the computer eliminates soldering, stripping, searching or waiting for vendors to supply needed parts. Inaccuracies caused by frequency and loading problems, expected when making direct measurements, are avoided. All significant variables are represented as time-varying voltages which can be scaled to be compatible with oscilloscopes, X-Y plotters and other recording devices. Design changes are accomplished simply by resetting potentiometers, ending delays in finding optimum components. Perhaps most significant, investigations beyond the state-of-the-art are simplified. The designer can "tinker" with component values above and below those available to determine their practicability.

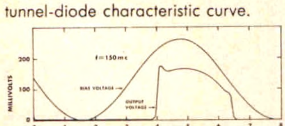
1000 MC Clock Frequency Study

A typical application is a small analog computer used to study the dynamic behavior of a tunnel diode switching circuit at various inputs and frequencies. The objectives were to determine the maximum clock frequency (rate of information transfer) of the system and to investigate the stability of the switching circuit which contained two negative resistance elements. Experimentation with the actual circuit was extremely difficult owing to the high frequencies.



Circuit Showing Reactances

Solutions on the analog computer (obtained in 50 milliseconds) showed that the circuit could conceivably be operated up to 300 Mc, but preferably at 150 Mc—vs. the 1000 Mc rate that preliminary paper investigations predicted. Use of variable slope/breakpoint diode function generators made possible the simulation of the intricate tunnel-diode characteristic curve.



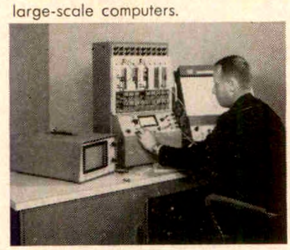
Output Waveshape of Switching Circuit

In addition to quantitative results, the simplicity of introducing parameter changes also permitted rapid optimization of conditions and regions of stability and the investigation of trade-off between gain (fan-in/fan-out capability) and frequency.

This problem is described in detail in Applications Bulletin ALAC 6348.

Tools for Model Building

EAI has two fully transistorized desktop computers which offer simple operation and programming. They require no special power or environment. The TR-48, a 58-amplifier computer, is capable of solving complex problems involving high-speed repetitive operation and iterative techniques. It is a sophisticated unit operable by a researcher or systems engineer with a minimum of training. The smaller and highly portable TR-20, the ideal starting computer, is also a successful research tool, capable of solving many problems formerly associated with large-scale computers.



A free computer operations course is provided with every TR-20 and TR-48 purchase at one of the company's computation centers. In addition, EAI offers tuition courses in areas throughout the country (send for details and date of next course).

Every new owner of an EAI computer automatically becomes a participant in the growing EAI Applications Library, and is eligble to receive simulation studies pertaining to all categories of research and design.

Send for detailed literature on EAI TR-20 and TR-48 analog computers, or arrange for a problem-solving demonstration in your plant or office—at no charge.

EAI® ELECTRONIC ASSOCIATES, INC., *Long Branch, New Jersey*

ADVANCED SYSTEMS ANALYSIS AND COMPUTATION SERVICES/ANALOG COMPUTERS/HYBRID ANALOG-DIGITAL COMPUTATION EQUIPMENT/SIMULATION SYSTEMS/SCIENTIFIC AND LABORATORY INSTRUMENTS/INDUSTRIAL PROCESS CONTROL SYSTEMS/PHOTOGRAMMETRIC EQUIPMENT/RANGE INSTRUMENTATION SYSTEMS/TEST AND CHECK-OUT SYSTEMS/MILITARY AND INDUSTRIAL RESEARCH AND DEVELOPMENT SERVICES/FIELD ENGINEERING AND EQUIPMENT MAINTENANCE SERVICES.

ELECTION AND PROMOTION OF INDIVIDUALS TO FULL MEMBERSHIP IN THE SOCIETY OF THE SIGMA XI BY THE CHAPTER-AT-LARGE

A. **Election:**

I. *Basis:* The policy of the Society for such action by the Chapter-at-Large is based on the knowledge that there are some persons who have attained recognition of their accomplishment in scientific research but who have been missed in the established procedures of the Society, for example:

1. Academic work was done at an institution where there was no chapter.
2. Scientific research accomplishments occurred after completion of academic program.
3. In exceptional cases, significant research accomplishments may have been achieved without the benefit of a formal advanced scientific education.
4. Eligibility overlooked by the appropriate institutional chapter Committee on Admissions.

II. *Criteria:*

1. A person who is a member of the staff or a student of an institution having a chapter is *not* eligible for election by this procedure.
2. The basic criteria for election will be such accomplishments as would justify election by an institutional chapter.

III. *Procedure:*

1. As the Society's procedures do not permit application for membership, a person before election must be proposed (nominated) by two active Members of the Society.
   a. Nomination is to be made upon standard *Nomination for Membership* form obtainable from Executive Secretary.
   b. Information called for should be complete.
2. Executive Secretary will prepare and send copies of nominations properly completed to all members of the Committee on Membership-at-Large.
3. Members of Committee on Membership-at-Large will indicate their approval or raise questions by mail ballot.
4. If approval is unanimous the nomination will go before next meeting of the Executive Committee in accordance with Article VII, Section 2(c) of the Constitution.
5. If not unanimous the nomination will be discussed at next meeting of the Committee on Admissions for the Chapter-at-Large.
6. The Executive Secretary will notify the nominee and nominators of the action of the Executive Committee. (In the case of referral to the chapter its usual procedure will be followed.) In case the nomination is not considered favorably, the nominators only will be notified.

B. **Promotion:**

I. *Basis:* The general basis for the promotion of Associate Members to (Full) membership in the Society will be:

1. The research work of a nominee done after his election to associate membership is of such a quality and amount that he is considered eligible

for election to (Full) membership, even if the nominee is not now actively pursuing independent research work.

2. The nominee has been and now is engaged in research work of a quality and extent that is considered by the Committee to justify his election to (Full) membership.

II. *Criteria:*

1. Associate Members who are either staff members or students at institutions having chapters, and Associate Members actually affiliated with institutional chapters, will *not* be processed for promotion by the Committee on Admissions for the Chapter-at-Large.
2. The basic criteria for promotion will be such accomplishment as would justify promotion by an institutional chapter.

III. *Procedure:*

1. An Associate Member before promotion must be proposed (nominated) for this advancement by two active Members of the Society.
   a. Nomination is to be made upon standard *Nomination for Membership* form obtainable from Executive Secretary.
   b. Information called for should be complete and nominators should indicate on form basis upon which nominee was originally elected to associate membership as well as subsequent data in support of promotion.
2. Executive Secretary will prepare and send copies of nominations properly completed to all members of the Committee on Membership-at-Large.
3. Members of the Committee on Membership-at-Large will indicate their approval or raise questions by mail ballot.
4. Nominations receiving notes of disapproval will be reviewed in meetings of the Committee.
5. If a nomination is not considered favorably, the Executive Secretary will notify the nominators, when this can appropriately be done, of the reasons why the nomination was not considered favorably.
6. In the case of a promotion approved by the Committee, the Executive Secretary will notify the nominee, with copies to the nominators.

C. **Initiation:**

Initiation shall be in accordance with Article IX, Section 2(b) of the Constitution.

D. **Public Announcement:**

1. *American Scientist* will be requested to report the election and promotion of Members by this procedure.
2. A press release will be made to proper media in each case of election and promotion by these procedures.

ELECTION AND PROMOTION OF INDIVIDUALS TO MEMBERSHIP BY THE CHAPTER-AT-LARGE

As Chairman of the Committee on Membership-at-Large, it is my pleasure to announce that under the provisions of Article VII of the Constitution, the Chapter-at-Large has promoted seven Associate Members to full membership and, with the approval of the National Executive Committee, has elected eight individuals to full membership.

The promotions were as follows:

Henry Lee Berryhill, Jr.
U.S. Geological Survey
Denver, Colorado

Badi Mansour Boulos
University Medical Center
Columbia, Missouri

Janusz Antoni Brzozowski
University of Ottawa
Ottawa, Canada

Delos Barker Churchill
Sperry Gyroscope Company
Great Neck, New York

Henry Howard George
Tube Turns, Division of Chemetron
Louisville, Kentucky

Leon Lorraine Hopkins, Jr.
Food and Drug Administration
Rockville, Maryland

Wayne Albert Stenback
College of Medicine
Baylor University
Houston, Texas

Those elected to full membership were:

Sanford Howard Ehrlich
Air Products and Chemicals, Inc.
Emmaus, Pennsylvania

Martin Irwin Goldenberg
U.S. Public Health Service
Communicable Disease Center
San Francisco, California

Dennis Howard Howling
Ledgemont Laboratory
Kennecott Copper Corporation
Wayland, Massachusetts

Stanley Buel Martin
U.S. Naval Radiological Defense Laboratory
San Francisco, California

Thomas Phillips O'Barr
U.S. Department of Agriculture
Beltsville, Maryland

Vincent John Sawinski
School of Dentistry
Loyola University
Chicago, Illinois

Meikyo Shimizu
Kaiser Foundation Research Institute
Berkeley, California

Graham Minor Sterritt
University of Colorado Medical Center
Denver, Colorado

The criteria and procedures, under which the Committee on Membership-at-Large acts as a Committee on Admissions for the Chapter-at-Large, were published first in the September 1964 issue of the AMERICAN SCIENTIST, and again in this issue, page 382A.

James H. Marks, Chairman

THE SCIENTIFIC RESEARCH SOCIETY OF AMERICA (RESA) PROCEEDINGS OF THE SIXTEENTH ANNUAL CONVENTION

October 6, 1964, New York City

The Sixteenth Annual Convention of the Scientific Research Society of America, RESA, was held in the Green Room of the New York Hilton, New York City, on October 6, 1964. The Chairman, Doctor W. E. Hanford, called the meeting to order at 2:10 P.M. The roll call, from the reply cards received from branch officers in response to the formal notice of the Convention, showed the following delegates present:

Natick Laboratory: Dr. Harold Hoge
Communicable Disease Center: Dr. Helen Casey, Dr. Max Moody
Naval Applied Science Laboratory: Dr. Walter L. Miller
Texaco Research Lab: Dr. Louis Roess, Dr. Richard Meyers, Dr. John Nolan
Ford Motor Company: Dr. E. C. McIrvine
System Development Lab: Dr. A. A. Gafarian
ESSO Research Club: Dr. J. E. Shewmaker

In addition to these delegates there were present: T. T. Holme, Leo Flexser, W. O. Baker, J. F. Weiffenbach, J. W. Copenhaver, D. L. Benedict, F. Rossini, and D. Young of the Board of Governors.

The minutes of the Proceedings of the Fifteenth Convention had been mailed to the branches and published in the March 1964, issue of AMERICAN SCIENTIST. No additions or corrections having been received, on motion it was

VOTED: to approve the minutes of

the Fifteenth Convention as printed.

The Chairman reported that the Procter Award for 1964 would be presented to Dr. Hugh S. Taylor at a dinner that evening in the Mercury Ballroom of the Hilton Hotel. Doctor Taylor's address would be on "Academia and Industry, Their Mutual Influence." Delegates who had not already made reservations were urged to attend.

The Director reported the following branch installations since the Fifteenth Convention: Wyandotte Chemical Corporation at Wyandotte, Michigan, on January 23; Kirtland Air Force Base at Albuquerque, N.M., on April 1; Dayton Air Force Base at Dayton, Ohio, on May 7; System Development Corporation at Santa Monica, Calif., on May 8; Magic Valley at Twin Falls, Idaho, on June 11; Thomas J. Lipton at Englewood Cliffs, N.J., on June 17; Central Texas Research Society at Temple, Texas, on June 23.

The following branches have been approved and await installation: Bolling Air Force Base, Washington, D.C.; AmChem Products, Ambler, Pa.; Hughes Aircraft Company, Fullerton, Calif.; Picatinny Arsenal, Dover, N.J.; U.S. Rubber Company, Wayne, N.J.; Westinghouse Electric Corporation, Baltimore, Md.; Xerox Corporation, Rochester, N.Y.; Youngstown Steel & Tube Company, Youngstown, Ohio.

The Director reported that the two meetings of the Board of Governors required by the Constitution of the Society had been held on March 11 and October 6 with good attendance. Minutes of these meetings are sent to branch officers.

The Treasurer reported that the Society's fiscal year is the calendar year and that an audited financial report for 1964 would be published in the March 1965, issue of AMERICAN SCIENTIST. An interim estimate indicated a small operating balance in the black for the year.

The Director reported a request from the Board of Governors that the authority to take final action on petitions for membership, both individual and group, be extended by the Convention to the Board for another year. The Constitution requires that all petitions for group charters, approved by the Board, shall be submitted to the next convention for action. This could lead to prolonged delay, nearly a year, for example, for a petition received soon after a convention has been held; and this might discourage a petitioning group. By extending authority to the Board only on a year-to-year basis, the delegates retain control of the situation. This plan has been in effect since the Society was founded. On motion it was

VOTED: that authority to take final action on petitions for membership, both group and individual, be extended to the Board of Governors until the next convention.

The Board at its morning meeting had tentatively approved a budget for 1965 based on a continuation of present fees; therefore, on motion it was

VOTED: to continue the present scale of fees through 1965.

These fees are: annual assessment per member—$2; first year fees for new members—$1 for an active member of Sigma Xi whose assessment to that Society covers a subscription to AMERICAN SCIENTIST, $2.50 for an inactive member of Sigma Xi, $3.50 for an entirely new member not a member of Sigma Xi, $25 for a club charter, $50 for a branch charter, and $50 for a life membership.

At the morning meeting of the Board of Governors, Doctor Hanford and Doctor Prentice were elected for one-year terms commencing July 1, 1965, as Chairman and Director-Treasurer respectively.

The Nominating Committee (Messrs. Starr, Coppoc, and Flexser) proposed to the Convention the names of D. L. Benedict, J. W. Copenhaver, and H. Heinemann for three-year terms on the Board of Governors commencing July 1, 1965. They were duly nominated, and there being no other nominations, on motion it was

VOTED: to elect Messrs. Benedict, Copenhaver, and Heinemann to three-year terms as members of the Board of Governors commencing July 1, 1965.

The new member-elect will be entitled to attend the March meeting of the Board.

In the absence of Doctor McKusick, RESA representative on the Sigma Xi-RESA Lecture Committee, Mr. Holme, Executive Secretary of Sigma Xi, reported on the plans and operations of the lecture program. As soon as a speaker has agreed to a tour in one of the nine regions, notices will be sent to all RESA branches in the area and dates assigned on a first-come first-served basis.

Doctor Taylor, Editor-in-Chief of AMERICAN SCIENTIST, had not arrived so Mr. Holme reported on this major activity of Sigma Xi and RESA. Statistics were given on the size of issues, etc., and it was stated that 106,000 copies (September issue) is about the limit of run on type and that the use of a rotary press in the near future is indicated. This change would require an enlargement of page size which might be a disadvantage in some ways to the reader but an advantage for our advertisers.

The Chairman asked the delegates for information on branch operations and for suggestions as to ways in which the central office could be more useful. One suggestion was made that agenda for the convention be mailed ahead of the date. This is required if any amendments to constitution or by-laws are to be considered and will be made general practice. The questions of increasing attendance at the convention and of a satisfactory date were raised by the Chairman, but no recommendations were proposed by the delegates.

There being no new business, the meeting was adjourned at 3:20 P.M.

Donald B. Prentice
Director

AN OPERATIONAL VIEW

By HUGH SKILLING

In these days we often speak of models. Scientists and engineers work with models, and by models we may mean anything from a conceptual model, which is an idea, or a mathematical model, which is an equation, to a plaster of paris model.

We hear about the Bohr model of the atom, or perhaps about other more sophisticated models. These are conceptual models, and they can perhaps be put down in mathematical form and become mathematical models. We hear about Newton's laws, which are mathematical models of mechanics, and we know that Einstein has improved on Newton's models by slight alterations that are important at relativistic speeds.

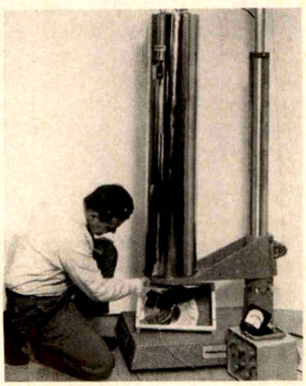
Models, of course, are based on observations. They are generalizations of a number of observations and measurements. All of our models—those of electrical behavior, of mechanics, of light, of heat, and any others—are generalizations of observations. This is by definition, for this is what seems to be meant by a model. It is very fortunate, by the way, that models can be conceived, for though we knew ever so many separate facts, as observed, but had no generalizations to work from, it is doubtful that we should be able to advance very much.

A model can be a theory, or a law, or a relation, or a hypothesis, or an equation, or a rule. We think of models having various degrees of probability; those called laws, like Newton's laws, are pretty likely to be dependable, whereas some new hypothesis that I have just invented is only a tentative model and is highly speculative. We have a good deal of confidence in a model that has been dependable through the ages. Newton's law of force and acceleration, for instance, has turned out to be right so many times that we willingly and literally risk our lives on our belief that it will continue to predict correctly what will happen next time it is used. It has been tried millions of times, and it has never failed once that I know of, except at quite high velocities; therefore I have great faith in it.

To move to the opposite extreme, I have considerably less faith in a new theory or model of the nucleus that tells of the structure of nuclear particles. This nuclear model is in a publication of last February, and I think it is not accepted by a good many physicists. I should say that its probability is not high. My faith in it is less.

Standard and Special Superconducting
MAGNETS AND MAGNET SYSTEMS

Magnion offers a broad range of standard and custom-designed superconducting magnets and magnet systems. For most applications, we recommend that the superconducting solenoid be specially designed to meet the specific experimental re-



Double-ended access, room-temperature working volume superconducting magnet system.

quirements. *There is neither a price premium nor a delivery delay for this service.* Typical delivery time for "custom designed" or "standard solenoids" is 30 days.

SPECIAL SYSTEMS

Among the special systems designed and built by Magnion are superconducting solenoids compensated to increase the region of field uniformity, special systems with the high-field volume isolated from the liquid helium environment, special systems for radial accessibility and transverse fields, and superconducting magnets with special iron structures for increasing the magnet's field, field shaping or improving uniformity.

Room Temperature Access Systems — Magnion double-ended access superconducting magnet systems (see Figure 1) provide a high degree of flexibility where room-temperature working volumes are required. The

Special iron-core superconducting solenoid for high fields at low cost.

system shown here is vertically oriented with ambient temperature access from both top and bottom.

Cryoflux Iron Core Superconducting Solenoids — The typical system shown in Figure 2, achieves a peak field of 75 kilogauss in a working volume of ⅛ inch diameter by ⅛ inch length. The groove shown on the side of the pole face provides access to the working volume. Without the removable iron structure, the solenoid has an inside diameter of 1½ inches and achieves a peak field of 45 kilogauss.

Compensated Systems — For the highest field uniformity, coil compensation is required. At 40,000 gauss a Magnion compensated 1½ inch bore coil provided a uniformity of 5 parts in 10^5 over a ⅜ inch diameter spherical volume, and 5 parts in 10^4 over a 2½ inch axial length. Even for applications with routine uniformity requirements coil compensation should be considered since it frequently can reduce coil size and cost by a major factor.

Vacuum-Chambered Working Volumes — Several systems have been delivered in which the solenoid bobbin was designed as a vacuum chamber to provide a working volume at pressures of less than 1 micron. Hermetically-sealed electrical feedthroughs supply contacts for solenoid and experiment leads.

Typical Cryoflux superconducting magnet system.

Cryoflux Cryogenic Systems — These are complete superconducting magnet systems, including dewar, header, current source, control unit and cryogenic bench. Standard Cyroflux systems afford controlled operation at temperatures from 4.2°K to 1.5°K. Magnion also offers a complete selection of Cryoflux accessories, including standard and custom dewars, power sources, liquid helium level indicators, fore pumps, manostats, stands, hoists and helium transfer tubes. For further information on standard and custom superconducting magnets and special cryogenic equipment write ..*Magnion, Inc., 144 Middlesex Turnpike, Burlington, Mass. 01804. Tel. (617) 272-5200*

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The important thing is that models, whether they are mathematical or conceptual or whatever, are not infallible. A good model is a good approximation of observed data, but it is not exact. Almost any generalization is an example of this; the ordinary rules relating temperature and electrical resistance are an illustration. Also, models have a limited range over which they are intended to apply; think of Hooke's law for elastic solids for instance. And models do not include all details that can be observed; their value as generalizations is in getting rid of obscuring detail. Newton's laws, if we return to them again, consider friction absent, and so they should, and yet friction cannot be avoided in any laboratory experiment.

Models need revision now and then. When more data are available, perhaps a model can be improved. Rutherford's atomic model was improved in Bohr's atomic model, and this in turn has been still further improved. Even Newton's laws were improved by Einstein after a couple of centuries.

Sometimes it is not a matter of improving but of completely remaking a model. Our ancestors' conceptual model of a world that was flat has given way to our own concept of a spherical world. And, of course, for some purposes the model must be even more exact than a sphere; perhaps a prolate spheroid or something better.

Really, what I know, what I have in my mind, is my own collection of conceptual models. Am I going to the District of Columbia? I run over in my mind my mental model of Washington. My mind contains a mental model of Washington, for I have been there before, and I have seen maps, and I have heard the city talked about. My model is limited, however, inaccurate, and quite incomplete in detail, for I have not been to Washington for several years, but still it is useful. It helps me go from place to place. I can see from my model that if I turn up Pennsylvania Avenue I shall soon pass the White House. And so, in fact, I do when I try it. My model passes the test of predictability. It tells me that if I do something, something will happen. And it does.

This is what models are for, I suppose. A model is a thing of beauty no doubt, but it is more than that. It can be a useful thing when it tells me that if I do something, something will happen. My mathematical model of Ohm's law tells me that when I connect a voltmeter across a resistance R carrying current i, the reading on the voltmeter will be equal to Ri. And so it is; my mental model has predicted correctly.

My mental model of this building tells me that there is a desk just beyond that door, and if I step across and open the door I shall see the desk. This I accept. I say this is true, and by saying "true" I mean that the probability of the prediction is really very high.

A "certainty" is something in which I have tremendous faith. I see a chair on the other side of the room. I say it is "certain" that if I were to step across the room I could sit on the chair. I do not really think that there is any illusion involved. I do not think I am dreaming. I do not believe that drugs have given me any kind of a hallucination, or that hypnotism is affecting me. I am perfectly willing to go sit on the chair with no expectation of landing with a thud on the floor. Why, I see the chair with my own eyes.

In this mental model of a chair I have great faith. And yet it *could* be wrong. What my mind knows is the mental model. This is brought by nerves, or so I believe, that are actuated by my eyes, as I understand the matter. In hundreds of trials, indeed thousands, and perhaps millions, I have been right in believing that my eyes reported such things correctly. Of course, there have been occasional misapprehensions when I believed my eyes and then found that things were not what I expected—or when I believed what I thought I saw, but was wrong. A dream, perhaps, or just a mistake. Anyway, there have been times when the prediction of what would happen based on what I thought I saw was not good.

I have a good deal of faith, though, in what I think I see. The probability is high that if I see a table, and I set something on that table, the thing will remain there where I expected that it would.

The predictability of my mental image is good.

Perhaps my most dependable mental models are built like this on what my mind tells me that I see, or hear, or feel with my fingers, or otherwise have brought in through the nervous system of my senses. However, I give a great deal of credence, too, to beliefs based on observations by other people, on authority. My own senses have never told me that the water of the Mississippi River is fresh water; I have never tasted the Mississippi water, or tested it. Yet I should be greatly surprised if I tasted it some day and found it salty. This is only one of countless examples in which I give practically as much belief to what I accept on authority as to what I accept from personal observation. I have never measured the radiation pattern of an antenna; I have never traveled by rail across Australia; I have never computed the orbit of Venus; I have never seen an iceberg—yet I believe I know something about each of these. Perhaps the instances in which I accept authority are even more numerous than are those of personal observation.

It is essential to what I am saying that my mental models, from whatever source they come, are never absolutely infallible. I believe in them, yes, and I act on my belief in them, and by doing so I get along happily. This, I think, is the essential point.

If I refuse to believe in what my eyes tell me is a tree right in front of me, and if I persist in walking straight ahead anyway, it seems highly probable that I shall very soon experience the feeling of a bump on the nose. The probability, I think, is high.

So there is some degree of probability associated with every one of my mental models. The probability is high that if I move a certain switch, a certain light will go on. The probability is fair that if I go through a certain series of actions my car will start. The probability is low, I think, that if I walk and watch the dip of a hazel rod I shall find underground water.

Everyone will agree, I think, that it is reasonable to relate some degree of probability with a law of physics. We are quite accustomed to think of the probability of a physical law being right. Such a probability may be extremely high. Possibly, Faraday's law of induction has been tried ten thousand times and has always been found to be right. Let us suppose so; suppose that it has never given the wrong answer. Its probability is extremely high. Still, the number of trials has been finite, and so its probability is finite. The probability that Faraday's law will give the correct answer next time is surely very, very high—but finite.

There seems to be no reason for not including in this same consideration the mental models that are based on what my senses tell me. The probability of a correct interpretation of what I see, I feel, I hear (or more accurately what I believe that I see, or feel, or hear) is high, no doubt tremendously high—but finite.

What with one thing and another, I have various degrees of confidence in my various mental models, ranging from a rather weak belief in some speculative models, through a firm faith in other beliefs, to a confidence that I call "certainty." I say that I am certain that chair is there, for I can see it. What I mean is that I surely think it is there; the probability is exceedingly high. I have a number of words that mean high probability, such words as "I am certain," or "I am sure," or "I know." When I say these phrases I mean that I have seldom if ever known an exception to something, that I consider an exception highly unlikely. Yet my experience, and all the experience on which I am counting, is finite, and is in some ways limited.

Can I be completely sure of anything, with perfect assurance? It seems that I know with complete certainty that I am thinking, and that my mind exists. Everything else is what I am thinking *about*. I believe something; I trust it. I have faith in it. Perhaps I have very great faith. For example, I believe that *you* exist. But as far as I am concerned you are really just my mental image. Perhaps I am yours, too. I hope so.

But this is extreme, and reminds one of Solipsism. Let us return, rather, to the more practical matter that to my

mental models, my thoughts, ideas, and concepts, I attach a sort of price tag that tells how much I trust them. Some I trust, as I have said, literally with my life. In others I have considerably less faith. I can easily think of examples and could mention various illustrations to cover the entire range of confidence from what I call "things I know," through "things I believe," to what I call "things I accept tentatively, dubiously."

The great value to me of this way of looking at my beliefs is that I do not expect too much of them. I see that every mental model has its limits. Whether it is my mental model of the solar system, or of Washington, D.C., or of that chair, it is based on finite observations. The chances may be a thousand to one that if I act on the assumption that my mental model is correct I shall find the results are what I expect, or ten thousand to one, or a million to one. Still, the chances are not beyond the number of times that the trial has been made, and surely this is the very highest estimate of probability that can be assigned, disregarding such possibilities as mistake, misapprehension, misunderstanding, or misinterpretation.

There is also to be considered the range of applicability of a model. The observations on which my mental model is based, whether they are my own observations or those of others that I accept, are surely limited in various ways, and I am not troubled when one of my mental models needs revision. The physicist of a century ago had faith in Newton's laws, but I hope his faith was not so great that he believed in their perfection even under untried circumstances. I hope he did not close his mind to the possibility of their revision when relativistic speeds became important.

No doubt one of my ancestors (and not too remote, either) had faith in his mental model of an earth that was flat. Surely his flat model had served him well on every single occasion when he had need of it. I hope he was not too troubled when the time came that it failed; when, perhaps, the sight of a distant ship receding below the horizon failed to agree with his flat model, and some kind of a change was required. His model was splendid for short distances, but it was limited. As a matter of fact, even now I use a flat model when I think of a trip across the city, though I must think of a round model if I plan a trip to Japan.

So it is a relief to me that I do not have to suppose that my mental models must hold true to the utmost bounds of space or time. What, indeed, *is* the utmost bound of space?—or of time? I do not know, and I am satisfied to say that I do not know. My mental models, my concepts, are not so unlimited. I do not expect them to be unlimited and, indeed, I think they probably cannot be unlimited. I do not feel any grief in recognizing that there are things I do not know, things I cannot know, and even paradoxes and inconsistencies among my models that I cannot set straight however hard I think.

I have suggested that the ability of a model to aid prediction is an indication of its worth. Suppose I want to go outdoors. My mental model of this building is valuable if, by its aid, I can walk in a certain direction, open a door, turn a corner, and find myself outdoors indeed. But this is perhaps a special case of a broader principle of the worth of models.

Perhaps a mental model is a desirable one to hold if it leads to good and wise results. If I want to go outdoors, the test of a good model is simple enough. A good model will show me what to do to go outdoors. But suppose I want to go to heaven, from which, we are told, no traveler returns. The ability to predict will not serve; it cannot be applied. It seems that I must now consider a model of heaven and the way to get there by asking myself the question, "Do I really think it is wise to accept such a model?"

Then, if I can say that the people who accept such a model are, on the whole, better off than another group of people who reject it, I will surely be wise to accept the model and add it to my collection of things I believe. Thus I find I am willing to say that a good model is one that it is wise to accept.

When I discussed these ideas with various people I was rather surprised,

although I suppose I should not have been, to find that objection came chiefly from the religious point of view. I had supposed it would be the other way; I had thought that this operational view, that gives the same validity to my religious ideas that it gives to my supposedly more mundane ideas of geography, say, or electricity, would be welcome. But it turned out to be not enough. I remember one man in particular; he belonged to a small but perfectly respectable sect. He required an absolute validity of religious teachings, so that the good people in the world could say to the bad people, "This is the truth. This is what you must believe and what you must do." I can see his point, and I recognize his view, and sympathize. Yet how do I know that he is not mistaken? Surely he could say, "My own experience, and that of others as far as I know, indicates that good things are likely to happen to people who do such-and-such, and bad things to people who do so-and-so." Thus, he may conclude that a group of people with certain religious beliefs are, on the whole, better off than another group of people with different ideas. With this I can agree. But if he wants to go further, I must confess that I find myself unconvinced.

Another moral objection to the operational view is easily overcome. "Do you mean to say," I am asked, "that we can believe what we like?" The answer is no, certainly not. I must believe what I consider it good to believe, wise to believe. This is not at all to say that, for the sake of ease or pleasure, I believe what I like. What I should like in that light sense, and what I consider good and wise, may be worlds apart. I might like to steal, or to cheat, yet I might consider such behavior to be neither good nor wise.

Surely no man can do better than to accept what he considers to be a good and wise belief. Can any religion show a better way?

There is an interesting corollary to this view of mental models. Suppose it is true that what I know consists of the ideas, the concepts, the mental models within my mind—and this I really cannot doubt. Suppose I find, as indeed I do, that these mental models are helpful to me, and that they guide me in all I do. Suppose I say that certain of my mental models guide me always, or nearly always, as I want them to do. Can I then consider that these mental models are close approximations of what is really and objectively true? This suggestion seems to need consideration.

What could I mean by "really and objectively true?" These words seem to refer to a universe external to me, external to my mind, to which I hope my mental models will approach if they are sufficiently refined. Now why should I believe in such an external universe? Why should I believe in the "reality" of an atom, or a table, of an x-ray, or a chair? What is this "reality?"

I talk and act and live as if I believed in the reality of a universe simply because it is tremendously helpful to me so to believe. If I were to act as if things about me had no reality, I fear that life would be uncomfortable and uncertain as I bumped into tables and tripped over chairs in which I did not believe. Therefore, I do indeed believe in what I choose to call the world of real things. It leads to successful predictions to guide me, and I consider it wise. Yet this belief in a universe of real things is merely a belief. It is a belief with high probability, certainly, but a belief none the less. I take the universe on faith; it would be unwise, I think, not to do so.

Indeed, it is very hard for me to give up the concept of reality. I am still inclined to say of a concept, "But is it really true? Is the nucleus of an atom *really* like this? Is it *really* true that the path of light curves in a gravitational field? Is it *really* true that space is finite but unbounded?" These are questions that I had better not ask.

Let me ask instead, "Shall I accept that the nucleus of an atom is like this? Is it well to consider that the path of light is curved in a gravitational field? Is it wiser for me to consider that space is finite though unbounded?" Is not this enough? Is not this, in fact, all that is possible?

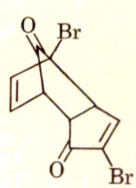
This operational view of ideas is not hard to accept on the plane of scientific theories. It is increasingly harder as we

deal with more common models. Yet there seems to be no line of partition at which one would stop (and hence the operational view is not Pragmatism). I can see no boundary beyond which ideas are beliefs, while short of the boundary they are facts. I can see only beliefs at all levels; beliefs with probabilities.

Finally, then, if these propositions are acceptable, and if you agree that what I know is within my own mind, and that, in acting on my beliefs, it is wise for me to accept some ideas and reject others, is there anything to be said by way of summary? Can I put it all in a few words? That I cannot; but I should nevertheless like to suggest, though it becomes more than a little cryptic, that in my opinion one can do no better than to believe what he believes it is good to believe. Can you say otherwise?

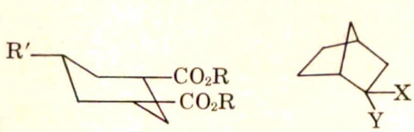
CHINESE CHARACTERS

In a letter to *Chemistry and Industry*, August 8, 1964, Dr. A. E. R. Westman of the Ontario Research Foundation, 43 Queen's Park Crescent East, Toronto 5, Canada, called the attention of the Editor to his impression that the organic formulae printed by the journal could be interpreted as a new form of Chinese writing. Specifically Formula II could be the character for a boy with a pea-shooter and XIV and XV could be entitled "but the villain still pursued her."



II

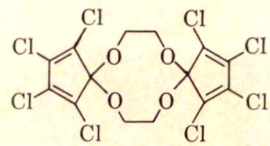
Boy with a pea-shooter



XIV–XV

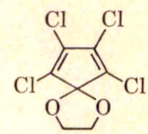
"But the villain still pursued her"

In response to an inquiry from American Scientist Dr. Westman has provided us with three more examples of his "Chinese Characters," on this occasion from Communications to the Editor, *Chemistry and Industry*, August 22, 1964, page 1491. The characters in question he labels respectively:



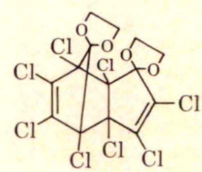
I

Skipping Rope



II

Hair Curlers



III

Dancing Cheek to Cheek

LETTERS TO THE EDITOR

Gentlemen:

The very enlightening article by Professor L. B. Slobodkin entitled "The Strategy of Evolution" (September 1964) contains an interesting fallacy, made all the more interesting since his concern with games overlaps with a dominant theme in the lead article in the same issue by Professors Simon and Newell.

Professor Slobodkin writes on page 348,

> Consider the analogies that are possible between evolutionary processes and changes on the one hand and games on the other. For example, imagine a group of non-chess playing observers at a tournament of mute chess players. It would be possible for such a group of observers to determine

the rules of the game fairly readily by simply observing the players.

It seems to me, because of the *inductive* nature of the situation, that one could not *in principle* (and *a fortiori* could not in practice) be *sure* of acquiring the rules for chess by just passively watching chess games. E.g., how would one be sure that a move didn't exemplify a strategy rather than a rule of the game? What distinguishes a strategy from a rule? Might not an "inductive learner" think mistakenly that certain moves are illegal according to the rules, when actually they are quite legal, but chess lore weights them worthless as a strategy? Similarly, a much used strategy might be mistaken for a rule to be obeyed by the most intelligent purely "inductive learner." For lack of information it seems that an "inductive learner" has no "decision procedure" to distinguish between a rule and a strategy. Games between "inductive learners" most likely have to be supplemented with metarules or metastrategies. These would, e.g., penalize a player who confused a rule with a strategy or *vice versa*.

Unless I'm mistaken, the preceding argument holds for the "games" of life, science, and possible even mathematics.

Sincerely yours,

Albert A. Mullin
Lawrence Radiation Laboratory
University of California
Livermore, California

In reply, Professor Slobodkin writes:

Dr. Mullin is certainly correct in saying that a universally beneficial strategy cannot be operationally distinguished from a rule by an inductive process.

This does not invalidate the general argument of my article since the distinction between a rule and a strategy that is followed as regularly as if it were a rule is only of interest in discussing the original intent of the game's inventor.

There is however an interesting point implicit in this criticism. By hypothesis, the whole apparatus of living organisms has evolved by the same process of maximizing flexibility.

In particular, the system of biochemical processes common to all organisms must have arisen in competition with alternative systems with flexibility as a major contributor to its success.

In any case, the game analogy, like any analogy is useful only until it has provided insight into our central problems and may best be discarded once that insight has been gained.

Yours sincerely,

L. B. Slobodkin
Department of Zoology
The University of Michigan
Ann Arbor, Michigan

"MESSAGE FROM A MEMBER OF THE COMMITTEE ON MEMBERSHIP-AT-LARGE"

At a recent meeting of our Committee, one of the members recalled an experience that is a good illustration of the type of problem this Society faces. Riding the train east he was talking with several doctors who had been affiliated with the Society at one time or another in the past but who now indicated they had lost touch with it and were unaware of what an active participating association could mean to them, to the Society, and to the country.

For years Sigma Xi has been concerned about precisely this question. Literally thousands of highly competent people have left school and lost their interest in affiliation. There has been no way to promote Associate Members to full membership, or to add new Members, once an individual has lost fairly direct contact with a local chapter. As a result of this, the Society's program to support qualified research and be active in national and local scientific councils has been impaired. Without continuing contacts with the Membership-at-Large, we often have lost the financial strength and impetus that could have been of great assistance.

In recent years significant efforts have been made to change this. The Committee on Membership-at-Large has developed election and promotion procedures which have been the subject of continuing informational releases to Members and chapters. These were published in the September issue of American Scientist and are reprinted on page 382A of this issue. While this procedure has just begun to function,

CHANGE OF ADDRESS

If your name or address has changed from that on file at headquarters at Sigma Xi or RESA, please return the form below to:

The Society of the Sigma XI
Office of the Executive Secretary
51 Prospect Street
New Haven, Connecticut 06511

This is to notify you of the following change of address or name. (Please print or typewrite.)

Former: Name . . . .
Street . . . .
City . . . . Zone . . . .
State . . . .
Zip # . . . .

Present: Name . . . .
Street . . . .
City . . . . Zone . . . .
State . . . .
Zip # . . . .

Affiliation: (Please give name of Chapter, Club or Branch)
- ☐ Chapter-at-Large
- ☐ . . . . Chapter
- ☐ . . . . Club
- ☐ . . . . RESA Branch
- ☐ Subscriber

Last Issue Received: . . . .

fifteen nominations have already been handled with appropriate promotions and elections being authorized. (These are reported on page 385A.) We believe this is a major step in the direction of strengthening the Society since it allows us not only to keep in contact with those who are not immediately associated with local chapters, but it also provides us with a way in which to recognize their achievements.

With this work accomplished, we intend to push ahead vigorously to encourage nominations for new membership and for promotion. We also intend to work enthusiastically toward other objectives.

In the first place, this is a service organization—it will be successful to the extent that individual members are interested and do constructive work in the Society and in their community. We believe there are at least two ways to do this:

First, each year the Society grants funds to individual researchers who need small amounts of financial aid. These are the important basic projects in the country that are not of sufficient magnitude or glamour to obtain grants from major foundations and other sources. Last year we had only about $100,000 available, although we had requests totaling $353,000. Clearly, one of the objectives of the Chapter-at-Large program is to improve the Society's ability to support this type of research. All members have an opportunity each year to contribute to the research program and we are planning to try to make these appeals clearer, more specific and more widespread. As an objective, the Committee on Membership-at-Large hopes to double in the immediate future the contributions for research grants and we look to all of our membership in the Chapters-at-Large for help in achieving this goal.

Second, in addition to the worthwhile cause of support of small research projects we are eager to be more active in stimulating efforts by our total membership to be effective in the scientific community and in the general community. We are considering pilot programs in coordination with specific local chapters which will allow an enlarged relationship between the chapter members and the Membership-at-Large in the community around the chapter. We can be helpful to the Society, and to the cause of improved scientific understanding and responsible decision-making in the community, if we can provide the tools for all of our membership to be informed about current issues and problems and to receive encouragement in their efforts to do something about them.

From time to time we will be reporting to you in these pages about the success of the Committee on Membership-at-Large in meeting these general objectives and about the specific things that each member can do to take more of an active interest. We welcome comments and suggestions and will deeply appreciate your assistance in achieving our stated goals.

Communication with respect to promotions and elections should be addressed to James H. Marks, Chairman of the Committee on Membership-at-Large, 51 Prospect Street, New Haven, Connecticut 06511.

Edmund A. Hartsook

BOOKS RECEIVED FOR REVIEW

TO THE MEMBERSHIP: *Our readers are reminded that this section of their journal is intended to be an information service listing first editions of the most important new scientific hardbound and paperbound books received for review in our Princeton Editorial Offices. Titles are for 1964 publication unless otherwise noted.*

From **Academic Press, Inc.**:

Identification of Materials, Via Physical Properties, Chemical Tests, & Microscopy by A. A. BENEDETTI-PICHLER; 492 pages; $18.

Differentiable Periodic Maps by P. E. CONNER & E. E. FLOYD; 148 pages; $6.50.

Analytical Methods for Pesticides, Plant Growth Regulators, & Food Additives, Vol. 3—Fungicides, Nematocides & Soil Fumigants, Rodenticides, & Food & Feed Additives, edited by G. ZWEIG; 237 pages; $12 regular, $10 to subscribers.

Analytical Methods for Pesticides, Plant Growth Regulators, & Food Additives, Vol. 2—Insecticides, edited by G. ZWEIG; 619 pages; $23 regular, $20 to subscribers.

Residues of Pesticides & Other Foreign Chemicals in Foods & Feeds, Vol. 6 of *Residue Reviews,* edited by F. A. GUNTHER; 165 pages; $6.

Advances in Morphogenesis, Vol. 3, edited by M. ABERCROMBIE & J. BRACHET; 408 pages; $14.

Mammalian Protein Metabolism, Vol. 2, edited by H. N. MUNRO & J. B. ALLISON; 642 pages; $21.

Structure & Metabolism of Corticosteroids, edited by J. R. PASQUALINI & M. F. JAYLE; 168 pages; $6; (Proceedings of a Symposium Held in Paris, July 1963 & Organized by the Faculté de Médecine, Laboratoire de Chimie Biologique).

Spectra-Structure Correlation by J. P. PHILLIPS; 172 pages; $6.

The Lunar Surface Layer, Materials & Characteristics, edited by J. W. SALISBURY & P. E. GLASER; 532 pages; $12.

Electronic Aspects of Biochemistry, edited by B. PULLMAN; 582 pages; $20.

Molecular Orbitals in Chemistry, Physics, & Biology, A Tribute to R. S. Mulliken, edited by P. LOWDIN & B. PULLMAN; 578 pages; $22.

Electron Microscopic Anatomy by S. M. KURTZ; 425 pages; $14.

The Bacteria, A Treatise on Structure & Function, Vol. 5: Heredity, edited by I. C. GUNSALUS & R. Y. STANIER; 517 pages; $16 regular, $14.40 subscribers.

Symptomatology & Therapy of Toxicological Emergencies by W. B. DEICHMANN & H. W. GERARDE; 605 pages; $18.

Recent Progress in Surface Science, Vol. 1, edited by J. F. DANIELLI *et al.* 414 pages; $16.

Microwave Scanning Antennas, Vol. 1, Apertures, edited by R. C. HANSEN; 442 pages; $16.

Physiology of the Amphibia, edited by J. A. MOORE; 654 pages; $18.

Biochemistry of Phenolic Compounds, edited by J. B. HARBORNE; 618 pages; 126s.; London.

Introduction to Infrared & Raman Spectroscopy by N. B. COLTHUP *et al.*; 511 pages; $12.

Nutrition, A Comprehensive Treatise, Vol. 1: Macronutrients & Nutrient Elements, edited by G. H. BEATON & E. W. MCHENRY; 547 pages; subscribers, $16.50; $18.50 regular.

Positronium Chemistry by J. GREEN & J. LEE; 105 pages; $5.50.

Scientific Uncertainty, & Information by L. BRILLOUIN; 164 pages; $6.50.

The Macromolecular Chemistry of Gelatin by A. VEIS; 433 pages; $14.50.

Physical Techniques in Biological Research; Vol. 5: Electrophysiological Methods, Part A, edited by W. L. NASTUK; 460 pages; $16.

Star Evolution, (Course 28, Proceedings of the International School of Physics "Enrico Fermi," Varenna, Italy, August 1962), edited by I. POLVANI, directed by L. GRATTON; 488 pages; $18.50.

Computing Methods in Optimization Problems, edited by A. V. BALAKRISHNAN & L. W. NEUSTADT; 327 pages; $7.50.

Advances in Insect Physiology, Vol. 2, edited by J. W. L. BEAMENT, *et al.*; 364 pages; $11.

The Physiology of Insecta, Vol. 1, edited by M. ROCKSTEIN; 640 pages; $22 regular, $19.50 subscribers.

The Microscopical Characters of Artificial Inorganic Solid Substances (Optical Properties of Artificial Minerals) by A. N. & H. WINCHELL; 439 pages; $14.50.

Carbene Chemistry, Vol. 1 of *Organic Chemistry* by W. KIRMSE, *et al.*; 302 pages; $9.50.

Proceedings of the International School of Physics "Enrico Fermi," Lake Como, Summer 1963, *Course 27:*

Dispersion & Absorption of Sound by Molecular Processes, edited by D. SETTE; 443 pages; *Course 29: Dispersion Relations & Their Connection with Causality*, edited by E. P. WIGNER, Director; 256 pages; 86 shillings for the latter.

International Review of Connective Tissue Research, Vol. 2, edited by D. A. HALL; 350 pages; $13.

Selenium (Geobotany, Biochemistry, Toxicity, & Nutrition) by I. ROSENFELD & O. A. BEATH; 411 pages; $15.

Analytical Methods for Pesticides, Plant Growth Regulators & Food Additives, Vol. IV: •*Herbicides*, edited by G. ZWEIG; 269 pages; $12.

Advances in Inorganic Chemistry & Radiochemistry, Vol. VI, (1964), edited by H. J. EMELEUS & A. G. SHARPE; 530 pages; $16.

Recent Progress in Surface Science, Vol. II, edited by J. F. DANIELLI, *et al.;* 541 pages; $18.

International Review of Cytology, Vol. 17, edited by G. H. BOURNE & J. F. DANIELLI; 401 pages; $16.

Physical Acoustics Principles & Methods, Vol. I, Part A: *Methods & Devices*, edited by W. P. MASON; 515 pages; $18.

Comparative Biochemistry, Vol. VII, Supplementary, edited by M. FLORKIN & H. S. MASON; 476 pages; $15.

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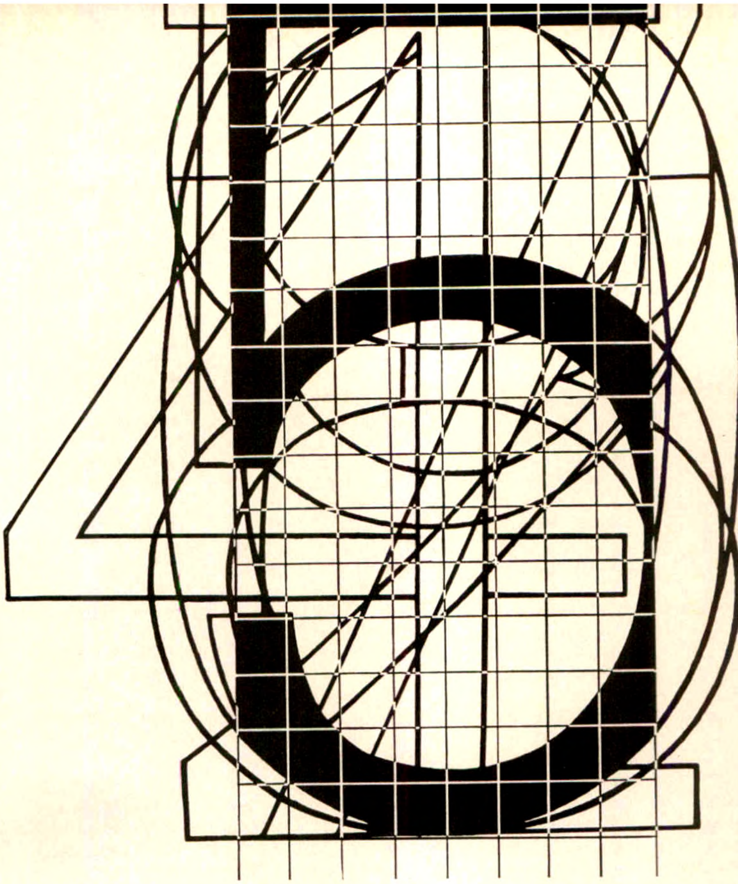

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Man-Made diamonds weighing two carats (9x).
Also Figure 7.

AMERICAN SCIENTIST, *52*, 1964

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MAN-MADE* DIAMONDS—A PROGRESS REPORT

By C. G. SUITS

THIS is, first of all, a story about carbon, the most aggressively gregarious element of the periodic table. Carbon, compounded, is so versatile in nature that the major branches of chemistry are determined entirely by the presence or absence of this element. Carbon, by itself in crystalline form, offers another distinctive dichotomy: it can be slippery, messy graphite worth a few pennies a pound; and, at the other extreme, it can be magnificent diamond, nature's hardest and most glamorous substance, sometimes valued at millions of dollars an ounce.

Converting a plentiful, cheap, and even worthless material into something rare and valuable was once thought to be a peculiar preoccupation of alchemists. If a play on words will be forgiven, we might say that this is now a worthy *occupation* for *all chemists*, all physicists, and all metallurgists—at least in industrial laboratories.

There have, of course, been some spectacular successes in this effort. Witness, as examples, the conversion of nearly worthless natural silicon found in sand to the highly valuable semiconductor-grade silicon on which a large segment of the semiconductor industry is based; or the conversion of cheap hydrocarbons—residues from coal, oil, and gas—to monomers and then to polymers of great utility and economic value. The planning, execution, and fruition of a successful venture in modern alchemy is one of the great satisfactions of today's scientists, technologists, and engineers.

It is not surprising that the history of science is replete with attempts to convert base carbon to noble diamond, and that the story is interlaced with claims and disclaimers, mystery and jealousy, suspense and intrigue.

The task itself is conceptually simple. Graphite is composed of carbon atoms, tightly bound in planes, with comparatively weak atomic forces

* Trademark of General Electric Company.

holding the planes together; the flaky character of graphite is the macroscopic evidence of this atomic structure. Diamond is composed of precisely the same carbon atoms squeezed together to achieve substantially uniform inter-atom distances throughout the lattice. Thus, the diamond lattice is a neat packing configuration which gives each atom a tight hold on each of the four atoms arrayed around it. In other words, to turn graphite into diamond, all one must do is press it into the more compact atomic arrangement of diamond, as is shown in Figure 1. What could be simpler? •

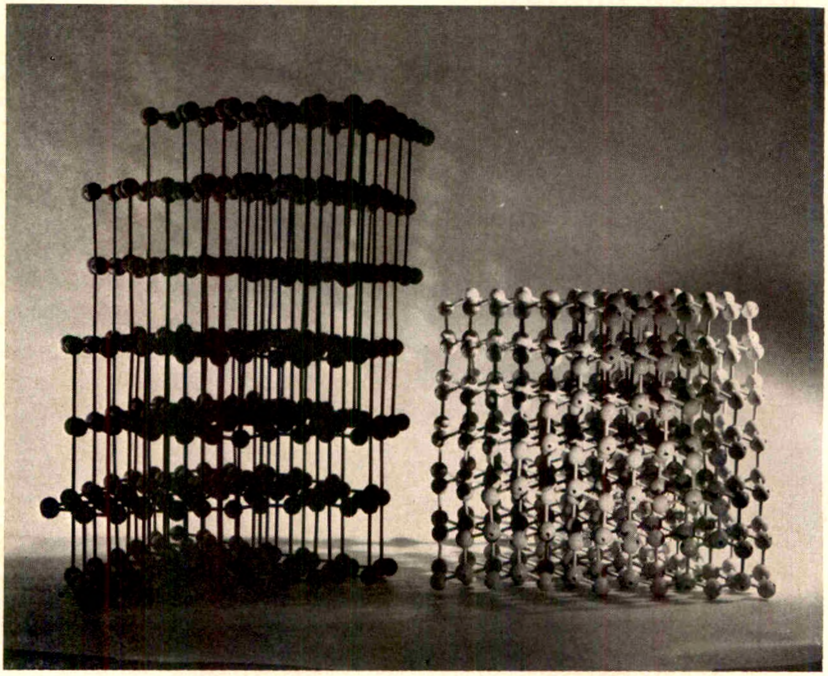

FIG. 1. Model of graphite lattice (left) and diamond lattice (right).

But, alas, as countless investigators over the years have learned to their agonizing dismay, Mother Nature did not intend her own achievement—probably performed at mysterious, unexplored depths far beneath the surface of the earth—to be easily accomplished by man. The late Professor Percy Bridgman of Harvard, a Nobel laureate for his work on high-pressure phenomena, put it succinctly. "Graphite," he said, "is nature's strongest spring."

Or, in the less grammatical phrase of one of our laboratory associates, it might be said that "it is easy to squeeze carbon atoms together, but very difficult to keep them squz."

Professor Bridgman spent many years trying to make diamonds during the 1920's, '30s, and '40s, and found serious natural roadblocks at every

step on this road. The most serious roadblocks were: (1) an understanding of the diamond-making process, and (2) the requirement of high pressure *and* high temperature simultaneously, both held for an appreciable interval of time. Bridgman produced pressures which were much greater than required, but the "spring" relaxed to graphite when the pressure was reduced. On the basis of what he learned, Bridgman concluded that all previous claims to success in diamond-making were based on wishful thinking and not scientific proof.

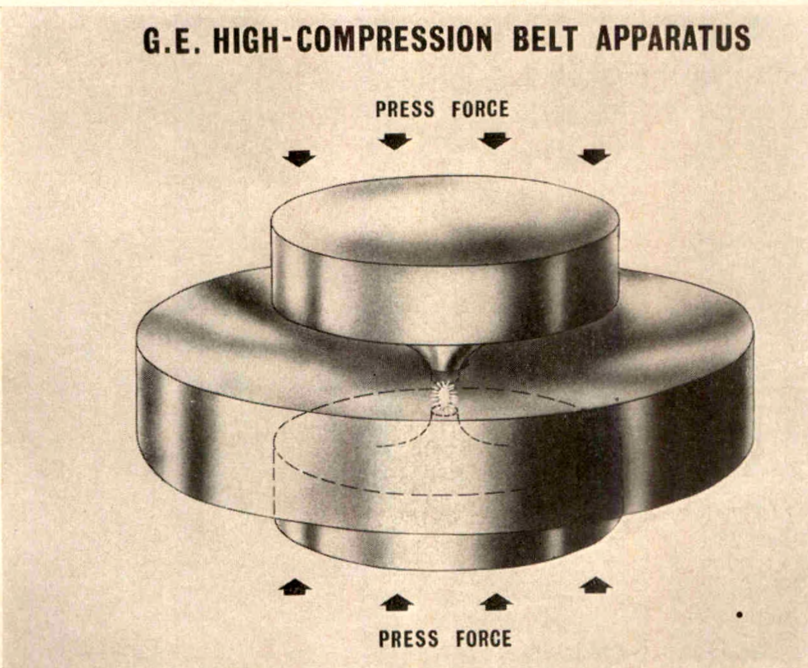

FIG. 2. Present form of the high pressure "belt."

In 1951, my associates [1] and I decided to launch a new, all-out assault on this problem. We resolved to delve into the process whereby diamond might be made, using not only new techniques for attaining fantastically high pressures, but also for attaining the simultaneous high temperatures that would be required to "latch" this spring: thus, to form diamond from graphite.

Let us now discuss three aspects of the diamond problem in turn: the pressure, the temperature, the chemistry—and then add a comment about a fourth factor: time.

Using the strongest available materials, unique pre-stressing techniques, and geometrically complex designs which permit the micro-flow of pistons and cylinders at extremes of pressure, the project to which we

have referred has developed equipment capable of withstanding pressures of up to 3,000,000 lb. per sq. in. This is 200,000 times atmospheric pressure, or, as the scientists put it, 200 kilobars. It is the pressure that would be found some 330 miles beneath the earth's surface. Or, expressed another way, it represents the pressure of a column of mercury 100 miles high, a rather substantial barometer reading. These astounding

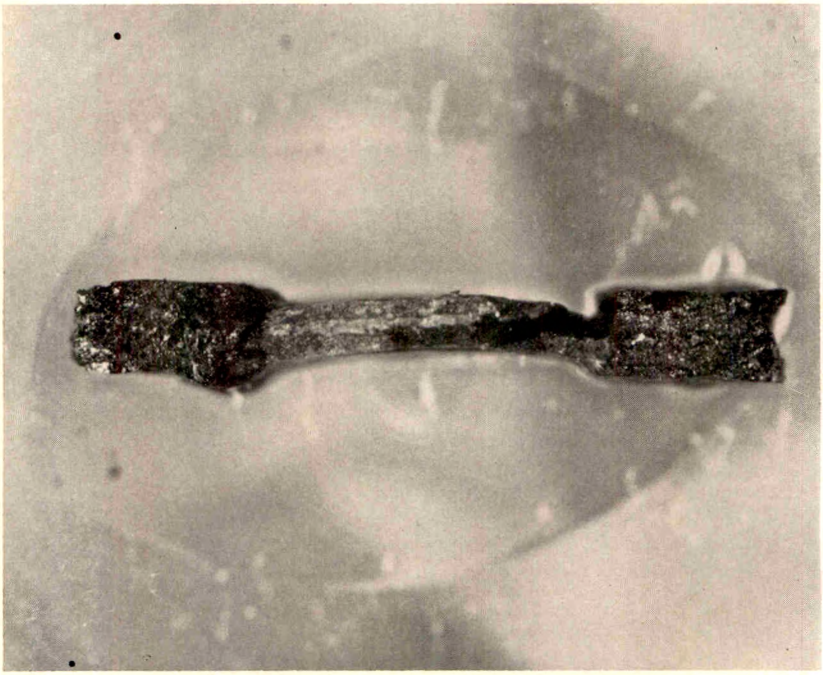

FIG. 3. Bar of graphite squeezed to diamond in its central section.

pressures, however, are not a record. Bridgman attained much higher pressures by far simpler means. The new progress, in apparatus, came from a chamber design that permitted the simultaneous attainment of high pressure *and* high temperature.

In terms of temperature, we have consistently referred to "5000 degrees." In the early days of diamond-making we meant Fahrenheit, and 5000°F. is still the temperature which can be held for "a long time"—hours if need be. More recently, temperatures of 5000°K.—about 9000°F.—have been achieved for periods of a few hundredths of a second in our superpressure chambers. This is nearly the temperature of the sun's surface.

The development of these pressure chambers has been described in detail on another occasion [2]. The present form of the high pressure

"belt" is shown in diagrammatic view in Figure 2.

Chemistry was at least as important to the initial achievement of diamond making, as the temperature-pressure combination. It has been found that certain metals, which are molten in the process environment, act as catalysts and greatly enhance the rate of the required change in the lattice arrangement. The net result of catalysis is a higher yield at a higher rate, at significantly reduced temperatures and pressures. Thanks to pressure, temperature, and chemistry, the graphite "stayed squz" and emerged as diamond.

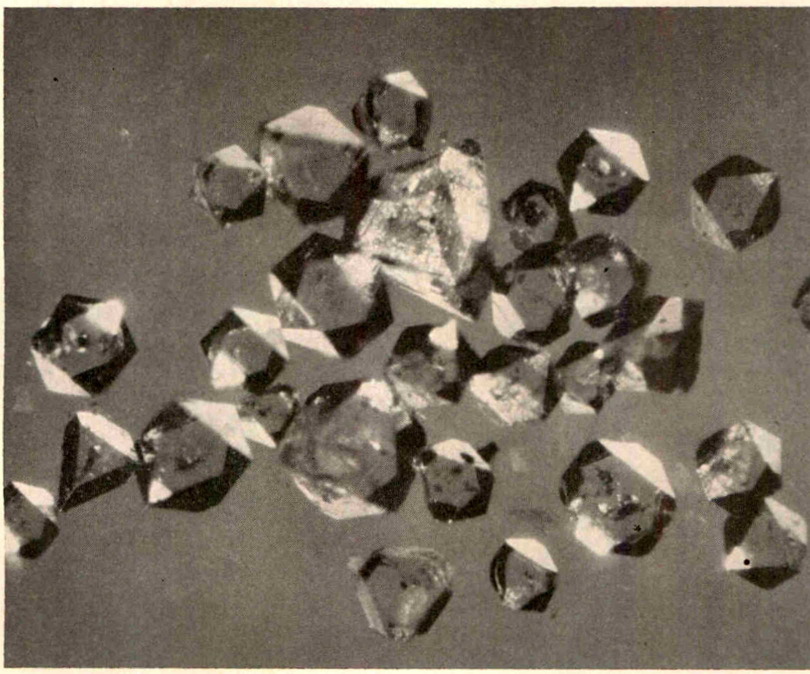

FIG. 4. Photomicrograph of early Man-Made diamonds (9x).

As we have seen, it is possible in superpressure chambers to hold the chamber conditions for long periods of time and thus to achieve a steady state for reactions under study. A temperature limit of about 3000°K. is set for such steady state use primarily by the available material properties, particularly the melting points of ceramic bodies. Under steady state conditions, say 1.5 million pounds per square inch and 1500°K., the diamond stable region of the carbon phase diagram is attained, but the diamond transition was not originally observed except in the presence of the catalyst. Francis Bundy showed, in static apparatus, by extending the temperature to 5000°K. for transient excursions, that the *direct*

conversion of graphite to diamond—that is, without catalyst action—takes place. This is illustrated strikingly by the photograph in Figure 3 of a bar of graphite which has been squeezed to diamond in its central section by these extreme transient conditions. (The density of graphite is 2.25; that of diamond, 3.52.) Still more recently, Bundy and Robert H. Wentorf have detected direct conversions at the upper end of the steady state temperature-pressure regime. And, the answer to the question of *time* is one of nature's bounties; the time is "short"—a matter of minutes or seconds, once reaction conditions have been attained.

The announcement that diamonds had been made at the General Electric Research Laboratory appeared in February 1955. To be honest,

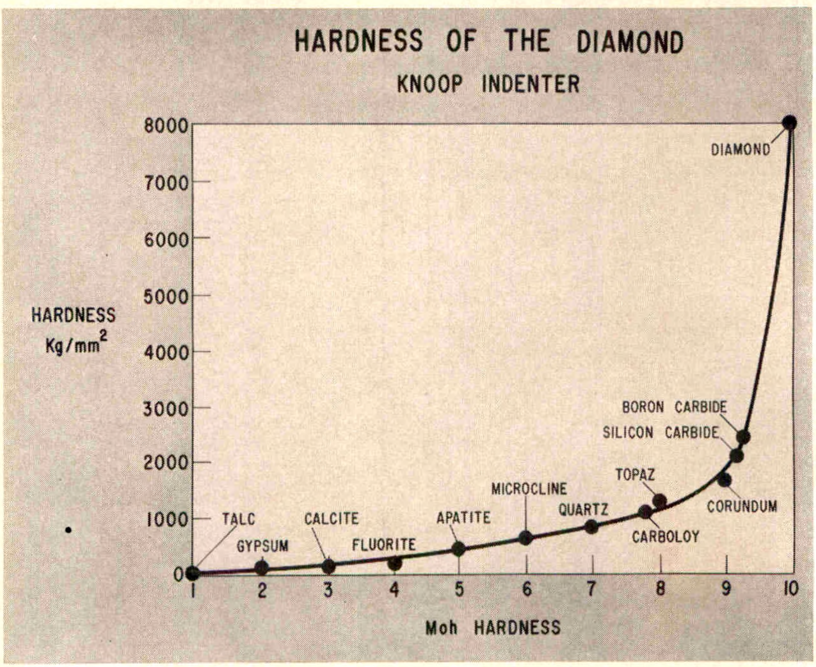

FIG. 5. Diamond is about five times harder than ruby and sapphire.

it should be noted that the diamonds made up to that time were small, as shown in Figure 4. Although they could be seen with the naked eye, it was better to use a microscope, and it was important not to sneeze, for that might have done away with the entire world supply at that time.

The initial reaction to this announcement was, nevertheless, spectacular. For example, on the stock market, the total value of General Electric stock increased that day by more than $300 million. Although this was an important discovery, the market reaction was in this case, as in other cases that might be mentioned, more emotional than analytical. To justify a $300 million increase in market price on the basis of a dia-

mond business, even with very favorable profit ratios, would require an annual sales volume greater than $3 billion, which was the approximate level of General Electric total sales at that time. The total world-wide sales of the diamond industry at that time were at about $1/10$ of this level. After a few days, during which such calculations were undoubtedly performed, the price of General Electric stock "recovered" to nearly its former level.

The announcement release in 1955 made it clear that Man-Made diamonds were small and were not of gem quality. One might jump to the

FIG. 6. Collection of one-quarter million carats of Man-Made diamonds.

conclusion that only diamond gems are really valuable. To put this question in proper perspective, one must bear in mind that the bulk of diamond production from all sources, worldwide, is in industrial grades with a total market value of about $60 million. The very much smaller physical volume of gem diamond sells at a very much larger unit price to achieve a world-wide sales level of about $300 million per annum.

A few points should be made concerning the *value* of diamond, the *price* of diamond, and some factors which determine both. Diamond has only one unique property—hardness—and this property is fundamental to its industrial use *and* its usefulness as a gem. However, hardness is a necessary but not a sufficient attribute of a crystal to be used as a gem.

As a gem, diamond has other important properties such as a high refractive index and must have other additional qualities, such as size, color, and optical clarity. But, without its exceptional hardness, diamond would not be pre-eminent as a gem. A higher refractive index is available in other crystals, especially titania, and large clear crystals of many other minerals are readily available.

Many persons have had the experience of wearing a ruby or sapphire gem in a ring for a period of years. A careful examination of the stone will show definite evidence of wear. On a true hardness scale, however, as Figure 5 shows, diamond is about five times harder than ruby and sapphire and it will accommodate the appreciable wear requirements of a gem stone. Thus, diamond is probably the only gem which can be used in an engagement ring which will survive the ideal marriage.

The same unique property—hardness—is the essence of diamond's industrial usefulness. This reduces in most cases to the ability, as in the case of a gem, to retain sharp corners under conditions of wear. The conditions of wear in industrial applications are, of course, much more severe than in engagement ring service. A suitable diamond saw can be used to cut concrete, for example, which it does, much as a steel saw cuts wood. Even more important, diamond wheels are used to cut the hardest and strongest metals and alloys—like Carboloy® cemented carbides—and the vital industrial usefulness of these carbides depends upon the availability of diamond for cutting these materials during manufacture.

Fortunately, diamonds do not have to be beautiful in order to be hard. The bulk of the diamonds dug from the earth are small, discolored, and not worth polishing, but their hardness is the same as diamond of gem quality. These poor cousins of the million-dollars-an-ounce gem stones are worth only a fraction of gem prices, which fraction, however, is still about $6000 a pound based on a typical diamond grit price of $2.65 per carat.

Soon after the discovery of the laboratory diamond process, we had to face the question: "Can Man-Made diamonds be produced that are good enough and inexpensive enough to compete with natural diamond bort?"

The answer came in a remarkably short time. A team of scientists, engineers, and manufacturing experts joined forces to make these Man-Made diamonds a competitive industrial product, and—as Figure 6 shows—this objective was achieved in less than three years.

With this final step, America acquired for the first time an independent source of industrial diamond, a material which is very important to the industrial economy, and which is absolutely vital in key defense industries. The assurance of a steady supply of industrial diamond has encouraged broader use of this exceptional material for new applications. And, surprisingly enough, the new Man-Made product has turned out to be better than natural diamond for many applications.

Man-Made diamonds are grown under controlled conditions, and remarkable control can be exercised over the properties of the tiny crystals. For some purposes it is desirable to produce friable crystals, so that fresh cutting surfaces will be exposed during use; such crystals can be grown. For other purposes—for example, metal-bonded circular saws—the ideal crystal would be an octahedron of the correct size; such crystals can be grown. This is an improvement on natural diamond because tiny crystals are not recovered from natural sources, and hence fine mesh-size,

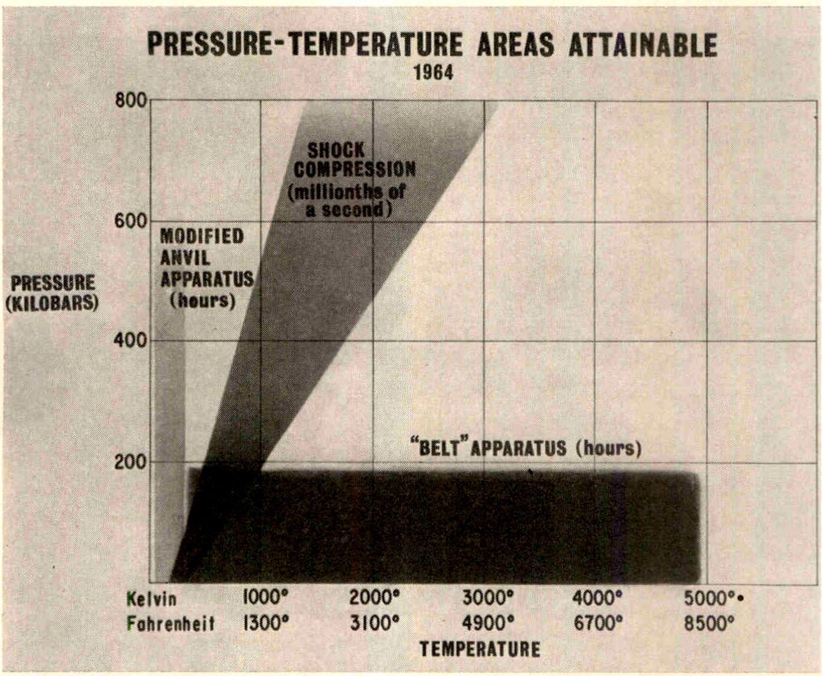

FIG. 8. Capabilities of different approaches.

natural diamond is generally produced by crushing, which does not readily yield the desired crystal shape. Thus, nature's hardest substance is now subject to quality control, which has significantly enhanced its industrial usefulness.

A "diamond mine," in Detroit, part of the General Electric Company's Metallurgical Products Department, is now one of the largest single sources of industrial diamond in the world. The product is made reliably and at a price that is directly competitive with natural diamonds. The total production of diamonds from this mine to date is not properly measured in carats, but in tons.

Most industrial diamonds, including Man-Made diamonds now on the

market, are very small: up to about half a millimeter in diameter and weighing only about a thousandth of a carat or less. However, diamonds in these small sizes in the form of abrasive grit fill a large portion of industrial needs. Meanwhile, considerably larger Man-Made diamonds—1 or 2 mm. long and of good quality—can be made.

Research continues toward the development of larger industrial stones, up to and including the carat-size diamonds required for oil-drill bits and wire-drawing dies. By growing the crystals in a multi-step process—adding a layer at a time—it has been possible in the laboratory to make diamonds weighing more than two carats, as shown on page *414A*. It must

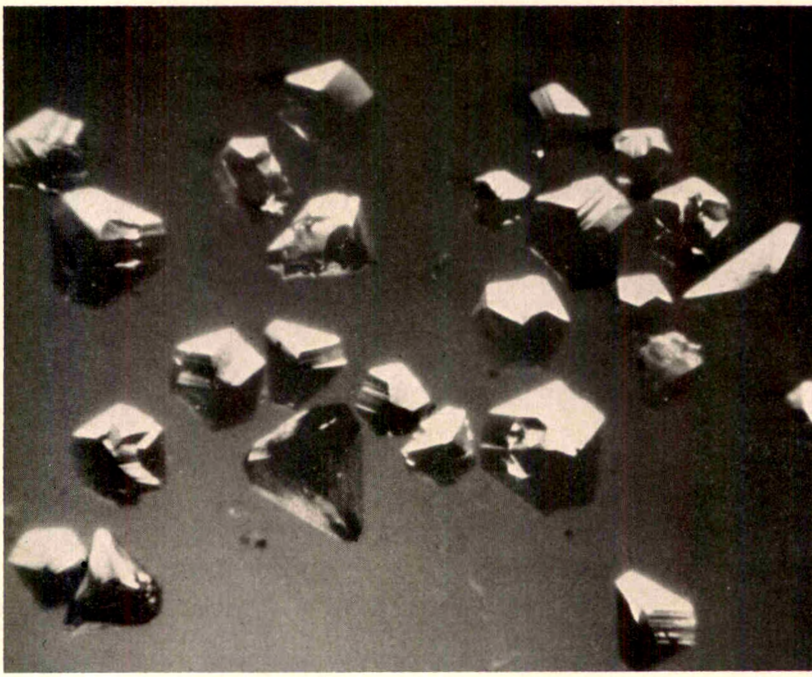

FIG. 9. Borazon—a completely new material never found in nature.(9x).

be noted, however, that these crystals are quite imperfect. They are not very strong because of inclusions, particularly between the layers, and because of internal strains in the crystal structure. They are definitely not the kind of clear, perfect stones suitable for polishing into something of interest to the ladies.

The scientific achievement of Man-Made diamond, and the commercial success of the product, have served to stimulate broad interest in high-pressure research at laboratories all over the world. Approximately 200 laboratories are now equipped for superpressure research, and much new useful knowledge of nature's physical extremes is coming from this work.

Many of the exploratory efforts in superpressure incorporate so-called "dynamic" or explosive shock techniques in which very high temperatures and pressures up to a million atmospheres are obtained for very short periods of time, usually a few millionths of a second. Diamonds have been made by these processes. Alternatively, very high "static" pressures, such as 500 kilobars, together with high temperatures, may be attained at a sacrifice of reaction volume, and some interesting researches are being conducted by this method. Many new forms of matter are being explored at superpressures, and it is clear that the final chapters

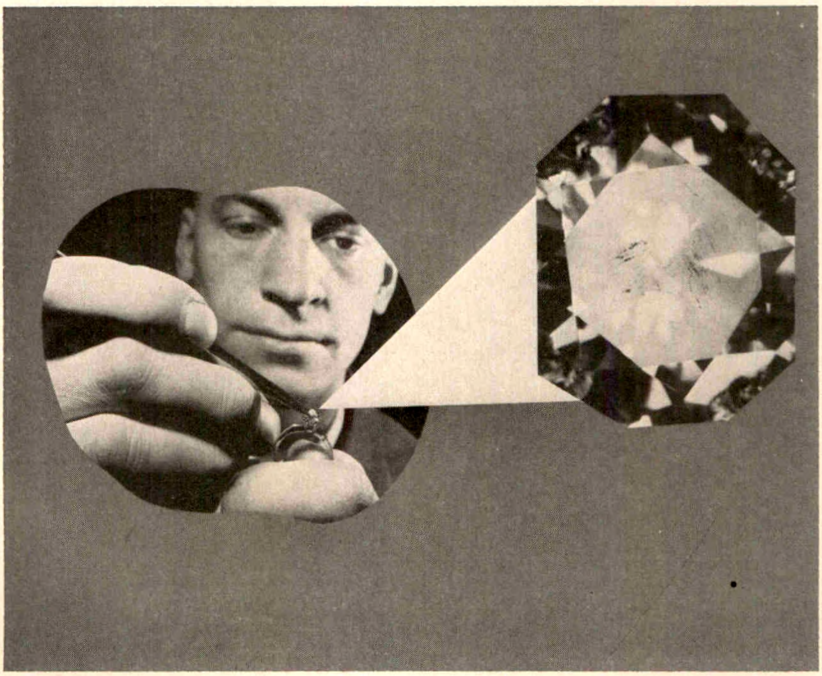

FIG. 10. Face of natural diamond gem stone (right) scratched by a piece of Borazon.

to the story of superpressure have not yet been written. One must expect exciting news from this source of scientific exploration for many years in the future.

The chart in Figure 8 shows some of the capabilities of different approaches. The "belt" apparatus developed at the General Electric Research Laboratory, because of its unique combination of 200 kilobar pressures and 3000 to 5000°K. temperatures—plus workable pressure-chamber volumes—has thus far provided an excellent diamond-making method as well as the research technique with greatest versatility. Superpressure research around the world is now concerned with such subjects as the determination of the elastic constants of various materials, the

properties of semiconductors at high pressure, nuclear magnetic resonance in solids and liquids under pressure, the determination of fixed points on the high-pressure scale, geochemical and geophysical studies, a variety of thermodynamic approaches to the study of activated processes under high pressure, and scores of other seemingly esoteric but inherently valuable research objectives.

In more down-to-earth language, it might be said that—obviously—it is possible to put many materials other than carbon into these cham-

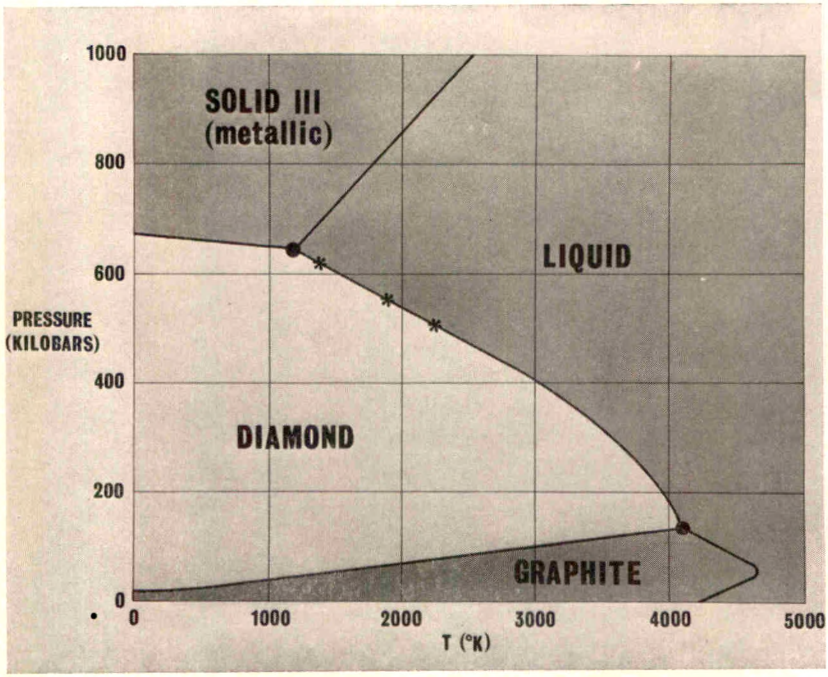

FIG. 11. Phase diagram for carbon.

bers and subject them to high pressure and high temperature for long periods of time. Such possibilities provide a virtually infinite challenge for the research scientist.

One of the first results of this kind of exploration was a new form of boron nitride. Boron nitride, in its common form, has a structure very similar to graphite, with boron and nitrogen replacing carbon in the lattice. It is a slippery material so much like graphite in mechanical properties that it is often called "white graphite." Boron nitride was sufficiently intriguing to prompt Wentorf at our Laboratory to try superpressure techniques on this material.

The result was spectacular: a completely new material never found in

nature (Fig. 9). "Borazon," as it was named, is in the same range of hardness as diamond; as the photograph in Figure 10 shows, it is the only material other than diamond that has ever been able to scratch diamond. Because Borazon is more oxidation-resistant than diamond, we believe it will eventually have important industrial applications. In addition to Borazon, more than 20 new forms of matter have been created through superpressure research in the one program with which I am most familiar. Principally, these are chemical compounds, although in some cases they are single elements converted into new crystalline arrangements. At present there is considerable scientific excitement in the Laboratory concerning evidence which points to the possibility of a completely new crystal form of carbon. However, this work is still incomplete, and requires confirmation. In the case of both germanium and silicon, we and other workers in the field have identified some new high-density forms substantially different from the crystal structure which helps give these materials their unique value as semiconductors.

But this was to be mainly a story about carbon, that many-faceted element which is so dominant in science and life. What is the future for *carbon* as we continue to heat it and squeeze it and catalyze it?

We are gradually learning how to make larger and larger diamond crystals—how to control the nucleation of crystal growth and achieve bigger single crystals with fewer occlusions and imperfections. We are learning how to keep unwanted atoms out of the carbon structure, and it seems reasonable to hope that stronger, more perfect crystals will result. Finally, we like to look at the phase diagram for carbon in Figure 11 and dream of the time when we can attain the presently unattainable areas of temperature and pressure. Perhaps carbon will assume a new form harder than diamond. In any event, crystallographers can conceive of atomic arrangements which might put carbon atoms even closer together—and more firmly bound—than they are in diamond. The result might be—who knows!

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AMERICAN SCIENTIST, *52*, 1964

COMPUTERS AND THE EVALUATION OF RESOURCE MANAGEMENT STRATEGIES

By KENNETH E. F. WATT

As more insight into the nature of ecological systems accumulated in the last two decades, it has become increasingly apparent that problems of managing renewable natural resources could not be solved by conventional mathematical means. During the same period, mathematicians and computer program designers, unaware of the problems in resource management, have been developing tools singularly appropriate for the solution of these problems, but for entirely different purposes. In this paper we shall explain why problems of systems management in agriculture, forestry, fisheries, wildlife management, and epidemiology contain characteristics prohibiting their solution using classical mathematical optimization techniques. Then we shall show why application of large memory, high speed computers, used in conjunction with modern programing aids and dynamic programing, make solution of such problems feasible.

The Immediacy of the Problem

Until the last few decades, there has been no need for sophisticated operations research type studies of techniques of managing resources. As recently as the early nineteenth century, for example, the only problem in connection with buffalo and sperm whales was how to catch more. There were about forty million buffalo in North America, being killed at the rate of two or more million a year (Roe, 1951), and sperm whales were being killed by the New England whaling fleet at the rate of about 4300 a year (Hohman, 1928). Similarly with agriculture, since the continent was still being opened up, and man was not pressing hard enough on his resources for competition from insects to be critical. Trees and fresh water seemed to be inexhaustible.

However, in 1963, the world looks very different. Most intelligent people concede that even the most plentiful resources can be depleted through over-exploitation, and that pest control is critical. For example, by 1910 the world catch of sperm whales had been driven down to one hundred and fifty-five (Clarke, 1954), and by 1888, there were about one hundred and fifty buffalo left in the U.S. (Roe, 1951). Other resources such as fresh water, sardines and salmon are watched with apprehension, and insect pests are the object of extremely sophisticated research. Particularly if we use trees for gasoline, the worldwide annual growth of wood will be exploited as fast as it becomes available. A recent critical study predicts that timber shortages will cause a revolution in construction techniques in the U.S. by the year 2000 (Lansberg, Fischman,

Fisher, 1963). The margin for error in long-term resource management planning is vanishing rapidly.

Characteristics of the Problems

Problems in resource management are typically identical in structure to those the mathematician calls "extremum" problems. That is, we are confronted with a complicated system which must be manipulated so that some variable of commercial interest is made to take a maximum or minimum possible value, subject to various constraints which restrict our ability to control the system, or set economic feasibility limits on the controls we can apply. Examples are a fishery, in which the yield of edible tissue must be maximized, or a forest, in which we wish to maximize the yield of useful lumber by minimizing the depredations of insect pests, and the effects of fire, disease, and wasteful inter-tree competition.

Many difficulties are encountered in the attempt to describe such systems mathematically, and manipulate the mathematical models to find the values of independent variables that yield the "extremum" value of the dependent variable (e.g., maximum yield of fish or trees, or minimum survival of insect pests).

Different types of resource management strategies have fundamentally different types of effect in space and time, and this renders difficult comparative evaluation of long-term effectiveness.

We will illustrate the different types of effects various management strategies can have by considering four basic types of strategy available for managing animal populations.

The first, and oldest technique is to effect an ecological change in the population itself. That is, we change the open season on deer, stock pheasant chicks, kill insects with insecticides, change the minimum legal length limit on game fish, or the international numerical quota on whales. Examples of newer techniques are electrified barriers to kill downstream-migrating lamprey larvae, and sex attractants to lure male moths to their doom, or "jam the radar" of the powerful sex scents the females emit. Basically, all these techniques are similar, in that their effects do not persist indefinitely, and do not disperse or magnify with the passage of time. Rather, they are compensated for by animal populations, because, if we remove members of a population, this only diminishes the intensity of the struggle for existence among the remainder who have resultant increased survival and reproductive rates. This is why insect pests are still with us, despite decades of treatment by a battery of powerful chemicals.

A second level of sophistication is represented by the attempt to change the numbers of some other organism which attacks the pest, game or food species of concern. We may seek to exterminate the mountain lions which kill deer, or introduce a wasp-like parasite into an or-

chard to kill a caterpillar. Aircraft may be used to spray a virus, bacterium or fungus over vast tracts of forest in order to kill a pest insect. These techniques differ from the first group because, in successful cases, their effects may spread in space and build up in time for twenty years or more (Smith, 1959). Also, if a pathogen, parasite or predator effect declines, it may flare up again if the pest undergoes rapid population build-up.

An entirely new Pandora's box opened up with the spectacular control of the Florida screw worm by the mass release of radiation-sterilized males. Generalization of this notion led to a host of new ideas about genetic manipulation of populations. Another example is the planting of splake, or speckled-trout X lake-trout crosses, in order to produce a fast-growing fish whose habits make it less vulnerable to lamprey predation. A more sophisticated notion is to release foreign races of Gypsy moths in America, in order to introduce reproductive difficulties of various types into the population (Downes, 1959).

Perhaps the most sophisticated control techniques of all have to do with environmental modification. Of course, fertilization of the land, ponds, lakes and arms of the sea is a venerable idea (Cooper and Steven, 1948). However, there are some very new techniques of this class. One is to grow plants specially to provide pollen for the adult stages of wasps that parasitize phytophagous larvae, thus increasing the carrying capacity of the environment for the parasites (Townes, 1958). Another idea is supported by recent research in population dynamics. Pests are most likely to build up to catastrophic densities where there are large, even-aged stands of trees of the same species, or only two similar species (Morris, 1963). Therefore, we can militate against pandemics by appropriate checkerboard logging operations, or by mixed-tree species plantings in orchards.

The availability of all these types of control renders strategy evaluation very difficult, because we have to compare procedures that are very different with respect to the frequency with which they are used, the conditions under which they are applied, the type of effect they produce, and the speed with which they produce it. Also, the economics of the different procedures is very different. Some methods are cheap, but take a long time to build up to maximum effect, and hence are best used as preventive rather than emergency measures; some methods produce spectacular but transitory results, but are sufficiently expensive and have enough undesirable side effects to be used only in emergencies. Particularly if we plan to use mixtures of such techniques, comparative evaluation is very complex. For example, in forestry it is feasible to withhold application of insecticides until a pandemic pest condition must be treated if foliage is to be saved. However, biological control agents could not be released in adequate numbers fast enough or cheaply enough over,

say 10,000 square miles, to be of use in this "emergency, save the foliage" type of control. Parasites and predators should ideally be released years in advance of a pest crisis, in order to take economic advantage of their self-spreading feature. This means that a sensible economic comparison is between preventive applications of some kinds of control versus emergency, short-term applications of other types of control. The implication for simulation studies is that we must compare, for various types of control, the cumulative sums of control costs and losses due to noncontrol over long periods of time, since the different strategies of timing of application do not allow for comparison on a short-term basis. Therefore, we need a mathematical model that simulates events over a sequence of many years.

Perhaps the single feature of natural resource management strategy evaluation that introduces the greatest analytic complexity is the fact that an event at a particular place may be the result of some prior event at *another* place.

Four examples from different fields illustrate this point. Spruce budworm populations do not build up at all points in a large area simultaneously. Rather, initial population buildup develops at certain epicenters, then high densities radiate out from these epicenters in wave-like fashion (Elliott, 1960). Other authors have shown that the epicenters for insect pest outbreaks can only develop at certain positions on the surface of the earth, and the location of these sites is determined by the interaction of topographic, site and meteorological factors (Wellington, 1954a; Wellington, 1954b.). Similarly, population fluctuations in fur-bearers are not in phase over wide areas, but show wave-like effects radiating out from epicenters (Butler, 1953). Catches of sardines off the coast of California in a given year are in part dependent upon the number of sardines spawned off the coast of Mexico two years previously, which, in turn, depends on behavior of the California current (Sette, 1960). Epidemics in human populations often show a characteristic pattern of migration from endemic focal points to distant points on the earth's surface. Bubonic plague, in particular, has repeatedly shown this pattern of slowly spreading from oriental focal points to proceed wave-like over parts of Africa, the Middle East, Europe, the British Isles and North America (Creighton, 1891; Link, 1955).

Therefore, in all these cases we are dealing with a complex historical process in which movement through space must be considered as well as the passage of events through time. Such processes could be simulated using conventional mathematical procedures if only one such type of process occurred at one time, and there were no other complications. However, several processes are changing through time, not all in phase at any position in space, and there are several other complications, as we shall explain hereinafter. Therefore, the only feasible way to mimic such

processes is to have separate models to mimic each point in space, or each square or cube in a two- or three-dimensional grid, respectively. The sheer bookkeeping effort involved in computing with such a conceptual system is out of the question unless we use an automatic bookkeeping system, such as FORTRAN or ALGOL (pseudoalgebraic computer programing languages) make available to us.

Mathematical Problems

However, the magnitude of the bookkeeping effort required in such problems only constitutes the beginning of our difficulties. Several other mathematical problems are raised by the following characteristics of biological systems.

(1) A complex ecological system (lake, forest, ocean, or farming area) is under the influence of a great many independent variables, all of which are liable to interact in subtle and important causal pathways.

(2) These independent variables may only be important if they operate at a certain time. For example, an egg predator or parasite has no effect once the host species has reached the larval stage; wind and ocean currents are critical at certain larval stages in the lives of fish and insects, but not at subsequent stages (Sette, 1943). Therefore, problems of synchronization should be considered in structuring models for use in evaluating resource management strategies.

(3) The same independent variable may enter several different causal pathways in a complex ecological system, and the end effects of some of these pathways may tend to cancel each other. For example, increase in temperature speeds development of a host, therefore diminishing the period in which it is vulnerable to parasitism; however, the parasites are made to search more rapidly by the same temperature increase.

(4) Inequational constraints must be built into the strategy evaluation model for biological as well as economic reasons. All biological entities are subject to restrictions of the following type: the organism can eat an amount of food per unit time up to, but not exceeding a certain maximum; increasing hunger increases the perceptual field within which a predator will attack prey up to, but not exceeding a certain maximum. Economic constraints arise because we can not control a problem in a resource at a cost that exceeds the economic return from the resource, or apply control measures to such an extent that economically undesirable side effects result.

(5) Large-scale biological mechanisms often exhibit cumulative effects, lags, and thresholds. Consider the case of animals eating perennial plants. The leaves of the plant compete for sunshine, so, up to a certain threshold level, consumption of the leaves by animals only reduces interleaf competition, and no permanent harm to the plant is done. However, consumption of leaves above this threshold level by animals exerts a

deleterious effect on the plants which cumulates from year to year, and which reveals its true extent only some years after irreversible damage has been done (Belyea, 1952). That is, we have a lag, as well as a cumulative effect.

(6) A most difficult problem in evaluating resource management strategies is created by the number of strategies available for evaluation. For example, an insect pest can be controlled, in principle, by different insecticides, pathogens, parasites, and predators, or by sex attractants, genetic modification of the population or ecological modification of the pest's environment. Each possible strategy can be applied at one or many times, at any feasible level of application, or in any combination with one of the other strategies. Consider the problem of managing the world stock of whales. What is the best way to distribute effort over species and lengths in order to ensure a maximum sustained catch? Clearly, some technique is needed for quickly eliminating that large proportion of the strategies in any situation which are sufficiently suboptimal to be unworthy of further consideration.

(7) Many resource management problems are very complex because they contain meteorological, technological, and economic components.

The Power of Computer Simulation

We simulate a phenomenon whenever it is cheaper than studying the phenomenon itself, or infeasible for some other reason to study nature directly. Recently, scientists in many fields have discovered the potential of mathematical experimentation using large digital computers in conjunction with the new pseudoalgebraic languages FORTRAN and ALGOL. These languages in effect constitute a new and very powerful type of mathematics that incorporates the features of many different branches of mathematics. They can describe functions, simulate logical processes, or describe matrix operations, through subscript notation. This last feature means that the computer can work with maps of populations on which a row X column grid has been superimposed. It is relatively easy to program the machine to simulate dispersal from one square to adjacent squares and whole maps can be printed on magnetic tape in a matter of seconds, or fractions of a second. Computers have many features that make them ideally preadapted for solution of large-scale problems in resource management. Tables of weather data for half a century can be stored in a fast-access memory (access time in the 2–10 msec range) and brought out for computing on each year's simulation in turn. Enormously complex relations between animals and the plants they eat, and their predators, parasites and pathogens can be simulated easily on a machine that does additions in 2–10 msec.

We shall illustrate typical present applications of computer simulation studies to resource management with three cases. The aim in discussing

these examples is not only to illustrate the capability of computer simulation for solving ultra-complex problems in resource management. Also, I wish to show that this new way of problem solving has the most profound implications for the character, organization, financing, and allocation of resources in science as a whole, and the education of scientists. In each of the three cases to be mentioned briefly, the following points will be noticed.

(1) Research has been done on whole problems, not on isolated fragments of problems. The four following points are corollaries of this one.

(2) Unlike much scientific work, in which an effort is made to strip a phenomenon down to bare essentials, these simulation studies attempt to mimic reality in detail, and therefore are massively complex. Complexity is not an issue with a machine that adds in 2 msec, can remember any one of 32,000 10-digit numbers in a similar time, and output data at 75,000 characters per second.

(3) All three studies are extremely interdisciplinary. The mathematical models used draw on data from economic studies, legal considerations, and meteorology, biology, engineering, irrigation, and other fields.

(4) An immediate implication of the preceding point is that simulation studies are typically the outcome of a long period of work by an enthusiastic, tightly organized team within which there is a very high degree of cooperation and a real esprit de corps.

(5) Since the models used are realistic, they consider movement of entities through space, as well as changing conditions through time. Thus, the fundamental character of the mathematical models used is dynamic in a spatial as well as a temporal sense.

(6) The people who construct the computer programs think and write about them in terms of computer languages, such as FORTRAN. In fact, the complexity of many of the models, due to their dynamic character, constraints, and immensely complex interacting systems would make description of the systems difficult or impossible in any other mathematical language. This point may have the implication that FORTRAN or ALGOL will become the Esperanto or Interlingua of science.

We will now consider three examples.

The first study was conducted by a team consisting of a lawyer, biostatisticians and economists at the University of Washington, on salmon gear limitation in Northern Washington waters (Royce, Bevan, Crutchfield, Paulik, and Fletcher, 1963). The role of the legal member was to determine if it is constitutional for the federal government and the State of Washington to restrict the number of operators exploiting a common property resource, if this was done in the operator's interests. He concluded it probably was. Therefore, the object of the simulation study was to determine the biological and economic effects of restricting the number of units of gear fished. An IBM 709 was programed to simulate,

in detail, the inland movements of sockeye, pink, silver, and chum salmon into the State of Washington and British Columbia waters. The four fish species were treated separately, as were four different categories of gear. The model mimics the behavior of the salmon with respect to all the waters they swim through, and the passage times. The spatial distribution of the gear, the length of time it is set, and all economic factors related to value of the catch, the operating cost and profit for each type of gear in each place were in the model. One thousand separate equations were included in the computer program. It was shown that the fishery would be more profitable if the number of units of gear were restricted. An idea of the nature of the results is indicated by the following specific conclusion. "For example, when the sockeye run was increased to four times the standardized level to give a total run of approximately 17,000,000 fish in convention waters and the gear intensity (the amount of gear) was reduced to one-half of the present level, the fishery managed to harvest 77.9 per cent of the run."

The second study was conducted by the Harvard Water Program (Maass, 1962). The team is a large group that, between 1955 and 1960, included about 50 people. Information from politics, economics, mathematics, statistics, power plant engineering, and other fields was built into the computer program, which, as in the preceding case, was written in FORTRAN. An IBM 704 computer was used. The problem posed in the simulation study was as follows. "Given: (1) a certain combination of reservoirs, power plants, and irrigation-diversion and distribution facilities; (2) target levels of irrigation and energy outputs; (3) specified allocations of reservoir capacity for active, dead, and flood storage; (4) a representative series of monthly runoff values, and 6-hr flows for flood months; and (5) a specified operating procedure—then by routing the available flows through the reservoirs, power plants and irrigation systems for an extended period of years, such as fifty, determine the physical outputs and the magnitude of the net benefits created." The computer simulated the behavior of some two hundred and fifty variables, and arrived at the optimal operating values for five reservoirs, irrigation and energy target values, power-plant capacities, and flood-control storage.

The third study was conducted by the Statistical Research Service, Canadian Department of Forestry, using data from many entomologists, foresters, and economists in the Canadian Government, and in collaboration with mathematicians at the RAND Corporation in Santa Monica (Watt, 1964). The object was to find out how best to control an insect pest in 10,000 square miles of balsam forest. The program was written in FORTRAN and run on an IBM 7090. Included in the simulation program were data on weather conditions over a thirty-five-year period, and equations describing the population growth of an insect pest, and its inter-relationships with the trees, parasites and pathogens. The operating

costs of any strategy selected were computed, as were the losses due to lost tree growth and tree mortality. Spread and buildup of parasites and disease were simulated. The 10,000 square miles were treated as six hundred and twenty five 4 mile x 4 miles squares, for each of which all computations were performed separately. Maps showing the distribution of pests, parasites, and incidence of disease were printed on magnetic tape, along with updated financial statements on timber losses, and operating costs of controls at the end of each year. Physiological parameters describing the pests, parasites, and epidemiology of the pathogens were included. The results have shed considerable light on the whole matter of pest control.

Thus we see that a powerful new tool is available for solving scientific problems in resource management of the utmost complexity. It is to be hoped that widespread use of the tool will cause many breakthroughs. However, there is a very real problem. Most scientific workers in resource management simply lack the background necessary in mathematics, statistics, and computing to exploit the new tool. Herein lies an enormous challenge for a new, interdisciplinary type of education. Traditionally, mathematics courses or science courses with considerable mathematical content have emphasized problem solution. The great complexity of the systems analysis required in resource management strategy evaluation studies is creating a need for greater emphasis on problem formulation and systems description.

In order to particularize the type of education needed, we must look rather carefully at the most recent trends in management of extremely complex systems. Whether we are talking about optimization of a space shot trajectory, flight path selection for an airline, routing of forms in an insurance company, the productivity of California deer herds, sardines, German forests, or blue whales in the Antarctic waters, we have certain fundamental problems in all cases. One of the most pressing of these is the plethora of strategies that are typically available for consideration. To this point, I have given the impression that this problem is solved by the availability of high speed modern computers. A moment's reflection will show that this is not true.

We have mentioned that resource management problems are dynamic. This implies that we are dealing with complex historical processes. Therefore, an action taken now has an effect on the future state of the system, which in turn will determine the decision which is optimal at a later time. An implication of this remark is that we are not interested in evaluating strategies on a "one-shot" basis, but rather we must compare the effects of various sequences of policies. To put the matter differently, policy A may be better than policy B now, but from the standpoint of the whole history of the system, A may *only* be optimal now if it is followed by B next time. Also, policy A may be optimal at time three in a sequence of

times, but it may be grossly suboptimal at time seventeen if it has not been used before. Consider, for example, insect pest control in a forest. If a parasite with the appropriate egg complement, searching ability, and intraspecific competition coefficient is released sufficiently early in development of a pest outbreak, releasing parasites may be the optimal policy. However, if we postpone action until the pest is in a pandemic state, we will not be able to get out enough parasites at reasonable cost to control the outbreak. In this case, insecticides would be called for to save the foliage on an emergency basis. Clearly, a specific policy may not be optimal at all times; some policies are optimal at certain stages in the history of a resource, and other policies are optimal at other times.

The best policy to follow policy A itself may not be A, but B, because A may be too expensive to apply repeatedly, or it may not be permissible to apply A repeatedly for some other reason.

Hence, we are forced into comparative evaluation of the optimality of sequences of operations over time, not the optimality of "one-shot" action. Consider the implications of this for the number of strategies to be tested, and the volume of computation required. Suppose we wish to explore the consequences of all possible combinations of M factors at N levels each over t times. At the first time, we would simulate MN experiments. For each of these, at the second time, there would be MN experiments. Proceeding in a simple trial and error fashion, there would be $(MN)^t$ sequences to compare with respect to their economic return, or "payoff." For two alternative courses of action tested at two levels each, for a sequence of four times, this number is

$$(2 \times 2)^4 = 256$$

For three alternative procedures, at three levels each, over four times, the number is

$$(3 \times 3)^4 = 6561$$

Clearly, this matter quickly gets out of hand, and for more realistic values, such as $M = N = 10$ and $t = 50$, we are beyond the range of the largest computers possible in principle.

Happily, Richard Bellman, a mathematician of the RAND Corporation, saw this problem on the horizon some years ago, and presented us with a solution (Bellman, 1957; Bellman, 1961; Bellman and Dreyfus, 1962). The solution depends on a very powerful principle: the optimality principle. This states, "An optimal policy has the property that whatever the initial state and initial decision are, the remaining decisions must constitute an optimal policy with regard to the state resulting from the first decision." This means that, at each time, we compute the states resulting from all possible combinations of policies and levels, and select the optimal one. *Only* this one is used as the input datum for the next time interval computation. The computational implication of this princi-

ple is that, at each of the t times, instead of considering MN cases for each of the MN cases at the previous time, we only consider MN cases for the *optimal* case at the previous point in time. The impossible computing load of MN^t cases has become MNt cases, a far more reasonable number!

Matters such as these are dealt with in a new branch of mathematics called dynamic programing, which is one of a whole new group of mathematical disciplines included under the general heading of operations research. Here is the terrible point however: to my knowledge, no foresters, biologists, botanists, zoologists, epidemiologists, entomologists, fisheries biologists, range managers, or agronomists anywhere in the world are exposed to these subjects as part of their required university training program; yet these subjects, like FORTRAN, are obviously of enormous potential value in resource management, which is how a lot of these people earn a living. May I respectfully submit that this is a problem worthy of serious consideration by the academic community. Clearly, there is urgent need for a complete revitalization of teaching programs in resource management, so that relevant new trends are included in the curricula.

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MATHEMATICS AND THE LAYMAN

By F. A. FICKEN

EVERY civilized person must study mathematics in school and, perhaps, in college and graduate school. And yet, unless he devotes a year or more of well-planned, full-time study to it, mathematics may remain for him a disorganized collection of nearly forgotten rules, tricks, and curiosities.

The layman consults physicians, clergymen, and lawyers. He appreciates directly the accomplishments of engineers, musicians, biologists, writers, and architects, but he knows little of what a mathematician does outside the classroom. There the layman may have gathered, moreover, that mathematics is a finished science, that its ideas have already been worked far past the point of diminishing returns. He may imagine that mathematical research involves elaborate technicalities with no basic ideas beyond those he has already met.

Nevertheless, mathematics always engages the most absorbed attention of some of the best minds of every epoch. It existed in Greece as a well-organized body of knowledge when Rome was a mere village and the rest of Europe was populated by pastoral barbarians. Recently, for the first time, applicants to the National Science Foundation for predoctoral fellowships in mathematics outnumbered those in any other science. The rate of publication of new mathematical results has more than doubled within the past decade and is now expected to *treble* within *five* years.

Mathematics is an exhilarating intellectual adventure. Mathematical thinking has a rather special character and a peculiar appeal to the human mind and spirit. I shall try to convey the flavor of these attractions. Let us look first at some mathematical accomplishments.

Mathematics deals largely with conditional statements, saying "if *this* is so then *that* follows (as a consequence)." Even in ancient days many scholars believed that the earth is spherical. Assuming this, Eratosthenes (240 B.C.) calculated the circumference of the earth; his error was less than 1%. Hipparchus (150 B.C.) estimated the distance to the moon; the error, traceable to an inaccurate measurement, amounted to about one part in seven.

A mathematical "if—then—" statement may express a physical prediction; here are two remarkable mathematical predictions. After Newton formulated his equations of motion and law of gravitation, late in the Seventeenth Century, the motions of the known planets were worked out. As time went on, predictions were improved and observations were refined. Comparison between prediction and observation

revealed, early in the 1840's, that the motion of the outermost known planet, Uranus, was not quite that predicted by the theory. An Englishman, Adams, and a Frenchman, Leverrier, working independently, speculated that the motion of Uranus was disturbed by a planet of unknown position and size, but still subject to the same general laws of motion as the known planets. Adjusting the equations of motion accordingly, they concluded by purely mathematical means that the unknown planet would be at a certain place at a certain time. An astronomer looked there then. He was the first to see the planet now called Neptune.

A still more astonishing prediction was made by James Clerk Maxwell just over a century ago. He first formulated equations describing the mutual interaction of electric and magnetic phenomena. In his equations he spotted a resemblance of these phenomena to the vibrations of a piano wire or a harp string. He knew that these vibrations have a periodic, recurrent character; they are therefore called waves. For purely mathematical reasons, Maxwell concluded that electromagnetic phenomena are periodic and that therefore electromagnetic waves must exist. He showed that visible light consists of electromagnetic waves of certain frequencies. He went on to predict that waves of other frequencies would be found. Over twenty years later, Hertz, a German physicist, produced in his laboratory waves of lower frequencies; they are now called radio waves. Soon Marconi began work on the technological problems and we were on our way to radio, radar, and television.

A negative prediction can yield real comfort. Consider the solar system. If we disregard the possiblity of intrusion by other celestial objects, and if our measurements are as accurate and our assumptions as valid as we believe them to be, then we can *prove* mathematically that the system can *never* suffer a catastrophe; no two bodies of the system can *ever* collide.

Simple curiosity prompts questions. The classical problem of "trisecting an angle" is to devise a geometric construction which, when applied to *any* angle A, will yield an angle equal *exactly* to A/3. Any bright youngster can construct an angle of 60°, and this angle is A/3 if A is the particular angle 180°. Many approximate constructions will yield A/3 from *any* angle A with an accuracy acceptable for any practical purpose. The problem is to construct A/3 *exactly* for *any* angle A. But there is more, and this is the real nub of the problem. The only tools allowed in this classical problem are a compass and an *unmarked* straight-edge. (Archimedes solved the problem with the conditions changed to allow marking the straight edge.) It was not until the Nineteenth Century that ideas were developed which made it possible to *prove* that, if a compass and an unmarked straight-edge are the only allowable tools, then *no* construction can yield A/3 *exactly* for *every* angle A. Curiously

enough, the construction is not possible if $A = 60°$, the simple and ubiquitous angle found in any equilateral triangle.

Our final example is not as technical as it may appear to be. It illustrates several typical modes of thought and leads to a striking result. The "perfect squares" are the squares of positive integers: 1 $(=1^2)$, $4(=2^2)$, 9 $(=3^2)$, 16, 25, 36, ..., 100, 121, 144, ... In ancient days mathematicians (and numerologists) were interested in sums of perfect squares. The celebrated *Theorem of Pythogoras* states that if a triangle has sides of lengths a and b meeting at a right angle, and the side opposite the right angle has length h, then $h^2 = a^2 + b^2$. For example, $5^2 = 3^2 + 4^2$. But this instance is atypical; while h^2 will always be an integer, of course, if a and b are, it may fail to be one of the perfect squares. Consider a right triangle with $a = b = 1$ (say); then $2 = 1^2 + 1^2$, and the integer 2 is not one of the perfect squares. Again, $3^2 + 4^2 + 5^2 = 50$, which is not one of the perfect squares.

Let us ask, therefore, which integers *can* be expressed as a sum of perfect squares. For example, $231 = 13^2 + 7^2 + 3^2 + 2^2$. The immediate answer is trivial; any positive integer n is the sum of n squares of unity:

$$n = 1^2 + 1^2 + \ldots + 1^2,$$

where there are n terms in the sum on the right. As sometimes happens, the question was hasty. After looking about a little we observe that if n exceeds 4 then four of the terms 1^2 in the previous sum can be combined to form 2^2. Similarly, $2^2 + 2^2 + 1^2$ can be replaced by 3^2. By combining, to form perfect squares, as many terms of the sum as possible, we are able to express n as the sum of a *least* number of perfect squares. For any fixed n we can obviously determine, by trial and error, the *least* number, call it $L(n)$, of perfect squares whose sum is n. What happens, now, to $L(n)$ as n gets large? If n happens to be a perfect square, say $n = m^2$, then $L(n) = 1$; there are arbitrarily large values of n, therefore, for which $L(n) = 1$. At the same time, however, the gaps between successive perfect squares get larger and larger, for $(k+1)^2 - k^2 = 2k + 1$ (e.g., $101^2 - 100^2 = 201$). It is an appealing idea, therefore, that values of n for which $L(n) = 1$ (or 2, or 3) are exceptional, and that if n is allowed to be very large there will be some very large values of $L(n)$. A remarkable theorem of Lagrange (1770) states that, on the contrary, $L(n)$ cannot exceed 4; *every positive integer is the sum of at most four squares of positive integers.*

Having seen a few achievements of mathematics, we renew our questions. What is the nature of mathematical activity? What is the quality of its appeal? To this end we look again at the typical statement "if *this* then *that*." The "this" is the *hypothesis* and the "that" is the *conclusion*. The statement says that the conclusion is a necessary consequence of the hypothesis.

Roughly speaking, now, one hypothesis will entail only rather few conclusions unless the hypothesis itself is quite elaborate. An elaborate hypothesis becomes more manageable if it is broken up into several hypotheses. We are thus led to consider the possibility of accepting several hypotheses at once and deriving their consequences. Mathematicians often do this. It is customary to call the hypotheses "axioms" and to refer to the conclusions as "theorems." Taken together, the axioms and the theorems they imply constitute an "axiomatic system."

Euclidean geometry, as studied in our high schools, is the prototype of an axiomatic system. One *axiom* states that through two points there passes exactly one straight line. Later on we shall say more about another *axiom*, the famous "parallel postulate"; in one of many equivalent forms it states that through a point not on a line there can be drawn, in the same plane, just one line not intersecting the given line. The Pythogorean *Theorem* was quoted in our last example. Another familiar *theorem* is that if two sides of a triangle are equal then the angles opposite those sides are equal.

In all axiomatic systems, as in Euclidean geometry, consequences are deduced from the axioms under the guiding principle that no two contradictory propositions can be accepted. If an inconsistency does arise, then the axioms must be adjusted to exclude it.

There are, of course, non-mathematical axiomatic systems; Spinoza's "Ethics" is an example.

Having cited Euclidean geometry as a typical axiomatic system, we hasten to enter a strong caveat regarding the status of the axioms. Thoroughly empirical in origin, they were at first regarded as self-evident truths, immediately verifiable assertions about physical objects. Even Aristotle, however, was prompted to ask what kind of a physical object is a "position without magnitude" (point), or a straight line that can be "produced to any length?" One can readily *imagine* a plane of unlimited extent, but to *realize* any such object physically is evidently out of the question. The points, lines, and planes of geometry are conceptual, not physical. Along with the realizability of the axioms we have also lost their "self-evidence" in the physically tangible sense of the word.

General acceptance of this attitude took many centuries. The story hinges on the "parallel postulate" mentioned before. This axiom is somehow out of character with the others. Euclid himself was reluctant to introduce it and, as H. S. M. Coxeter has pointed out, thus qualifies as the first non-Euclidean geometer. Many mathematicians devoted their most strenuous efforts, without success, to seeking a proof that the parallel postulate is a consequence of the other axioms. About 1830, the whole situation became quite clear. As seems often to happen, the time

was ripe. Three mathematicians (Gauss, Bolyai, and Lobatschewsky) in as many countries at nearly the same time, produced entirely consistent geometries in which the parallel postulate is false. In non-Euclidean geometries it may happen that *any* two lines intersect, or that through a point not on a line there may be drawn *more* than one line not intersecting the given line. On the *surface* of a sphere, for example, the "straight lines" are great circles; any two of these intersect twice. The non-Euclidean geometries have important applications in a wide variety of situations including, for example, navigation, relativity, and the emerging theory of binocular space perception.

Beyond these practical applications, the non-Euclidean geometries have profound intellectual significance. They destroyed completely the idea that there is any "self-evidence" about axioms, or any "truth" in a physical sense. No matter how empirical in origin or practical in purpose, a collection of axioms brings into being a fully-organized conceptual system having, as von Neumann put it, "a peculiar life of its own." The terms used in the axioms have precisely those properties stated in the axioms; no more, and no fewer. A term borrowed from popular language takes on a strictly technical meaning. The popular meaning of the term may be helpful but often, as with the word "imaginary," it may be completely misleading. In any event, no appeal to the popular meaning is permissible in arguing deductively from the axioms.

These conceptual structures, fortunately enough, are extremely useful in attacking problems arising in science and technology. Statements and questions about a concrete system may use terms and relations which can be brought into quite detailed correspondence with the terms and relations in some mathematical system. It may then be possible to formulate a purely mathematical problem whose solution can be interpreted in terms of the concrete problem. It is fairly clear in this context, incidentally, what it means to say that "mathematics is the language of science." The aim is to find a mathematical system (now often called a model) which reflects the behavior of a concrete system (a physical or biological system, or a human enterprise) in such a way and to such an extent that study of the mathematical system can yield insight into or information about the concrete system.

By far the richest constellation of mathematical models is found in the various branches of theoretical physics. Newton's laws of motion may be regarded as the basic model of the classical mechanics of a particle. A body falling freely near the surface of the earth receives a constant gravitational acceleration. Strictly mathematical methods (very simple methods if air resistance is ignored) can be used to obtain from this fact quite precise information about the behavior of the body. If we know the height from which it is dropped, then we can calculate

its position and speed at any subsequent instant, can find out how long it will take the body to fall through a given distance, can predict when it will hit the ground and how fast it will then be travelling, and so on. Results on the mechanics of particles and rigid bodies are basic to all other mechanical studies. They underlie the general theory of dynamical systems on which the astronomer bases his precise predictions. In another direction one arrives at the mechanics of matter which is regarded in the model as being continuously distributed; here one studies deformations of elastic bodies and the mechanics of liquids and gases, including the picturesque phenomena of compressible flow. In another direction one arrives at thermodynamics and the statistical theory of heat. Still another path leads to the quantum-mechanical behavior of the atom and its nucleus.

Economics and other behavioral sciences are finding novel uses for familiar mathematical models and are requiring, moreover, the development of many new models, including some of quite unexpected character.

Mathematics has been called an "exact" science. Models of concrete situations, however, as well as purely mathematical considerations, sometimes require thinking that appears to be somewhat inexact. The model may lead to several equations for which a simultaneous solution is sought. It may be possible to show that (an exact statement) there is no solution; then the model is presumed to be an unfaithful reflection of the concrete situation and adjustments are contemplated. It may be possible to show that the desired solution exists, and even that there is no other solution. These are also exact statements. At the same time, methods of calculating the solution may be *inherently inexact;* all one can do then is to approximate. At this stage the mathematician must try to prove exact statements about inexactness. He must *estimate*, state precisely his estimate and the conditions for its validity, and then give argument establishing its validity under those conditions. If a certain process of approximation is used, what is the error? If the process can be improved in a systematic way, after how many improvements will the error be reduced below some acceptable margin? If several processes are available, which one will make the error least?

Whether or not a solution can be calculated exactly, special interest attaches to appropriate questions which, though quantitative, contribute to a qualitative appreciation of the behavior of the solution. How large can it get? How close to zero can it get? Is it always positive? How does it change when various features of the model are changed? How sensitive is it to fluctuations of the data from which it is calculated? Does it vary periodically with any of the independent variables?

Because of its myriad applications mathematics is often developed for practical reasons. Even when working on a mathematical model of a

concrete situation, however, a mathematician spends most of his time immersed in mathematics, pausing now and then to verify the relation between what he is doing and the concrete situation. When so immersed he is largely unconscious of practicalities, attentive only to the mathematical situation confronting him immediately. This activity would proceed equally well, for some time at least, regardless of any practical relevance. It is therefore entirely natural that much valuable activity is prompted solely by intellectual curiosity, with no reference to any potential application. The internal strength of the science depends in a most essential way on mathematics done for its own sake.

Time and time again, moreover, purely mathematical results have had most unexpected practical value. Ideas which Riemann first broached over a century ago culminated a few decades later in the tensor calculus of Ricci and Levi-Civita. Devoid at that time of practical relevance, this calculus was indispensable to Einstein in his development of the theory of relativity, and has also found numerous applications in other branches of physics and engineering. It is now widely appreciated that vigorous activity in pure mathematics is essential to our national welfare.

A mathematician may now seem to be one who formulates systems of axioms and studies their consequences by proving theorems. This picture is grossly over-simplified. It is true that much significant mathematical activity does take place in tidy proximity, so to speak, to some quite specific set of axioms. Much more typical, however, is a kind of tentative conceptual reckoning.

A mathematician has in mind a highly organized body of concepts, interrelated in patterns of rather intricate subtlety. He finds certain of these relations puzzling. He reflects on the situation, probing now one aspect and then another. He takes a walk, looks eagerly into the literature, writes a few sentences or does a few calculations, stares out of the window, picks the brains of students and colleagues, and thinks with the absorption peculiar to creative effort. His powers of intuition and reason are engaged simultaneously, in a fusion so profound that the distinction between them is evanescent. He seeks a hitherto unperceived relationship and, simultaneously, threads of argument that can be coaxed into a pattern from which the relationship will emerge with that indubitable evidence required in mathematics as a credential for acceptance.

Although they do determine the general context, the underlying axioms may well be at a great distance, so to speak, from the arena of the immediate endeavor. There may be many intervening auxiliary assumptions, narrowing and specifying the context but, at the same time, enriching it to such a level that quite comprehensive elaboration of detail becomes possible. Confronted with this welter of potentialities

the mathematician ponders tentatively. "Here are several situations with a common feature; how shall I formulate it? Are all those assumptions required to prove this result? The desired conclusion is nearly established; can I get it by sharper use of the assumptions I have, or is some further hypothesis necessary?" The ultimate product, as it appears in the professional literature, usually seems quite reasonable, perhaps even natural to the point of obviousness. Such finished elegance results, as often as not, from an imaginative, open-ended, catch-as-catch-can, creative struggle with refractory ideas.

A mathematician is therefore a builder of conceptual theories. The axioms are the foundation; the theorems are the superstructure. He is an architect of ideas. His work is creative, with intense excitement and no little agony, resembling in its emotional demands that of a painter, composer, or writer. The latter comparison is by no means superficial, for to invent and communicate mathematical ideas involves a highly specialized linguistic apparatus. This apparatus deserves notice.

To a layman the symbolism and technical vocabulary of mathematics (briefly, our lingo) quite naturally appears to be very formidable indeed. The whole discourse has an occult flavor quite alien to the vernacular.

Each discipline has its lingo. Often, however, as for example in biology and organic chemistry, much of the jargon is recognizably taxonomic in aim and does not dominate the discussion. In such subjects, few adjectives and fewer verbs and adverbs have technical meanings, and for this reason (perhaps) popular presentations may be feasible. But popularizations of mathematical themes are rare. Even a professional mathematician may find it genuinely difficult to master enough of the lingo of an unfamiliar area to understand current work in the area.

And yet, it is easy to see how the lingo originates and how indispensable it is. The axioms contain words or other symbols which designate objects whose mutual relations are specified by the axioms. As the consequences of the axioms are developed, complication mounts and abbreviations become convenient. Precisely designated technical meanings are therefore assigned to appropriately chosen words and other symbols. One agrees, for example, to say "equilateral" triangle instead of triangle "with three sides of equal length." The symbol "0" has a myriad meanings, each perfectly clear in its own context.

(Added in proof: The September, 1964 issue of the "Scientific American" (Vol. 211, no. 3) is devoted almost entirely to mathematics. Several of our remarks are anticipated there, often with greater detail. A striking illustration of the conceptual necessity of abbreviation is given on p. 118 by W. V. Quine. The opening pages of F. J. Dyson's article illuminate the idea of a mathematical model in physics. Succeeding papers deal with contemporary models in other areas of science

and technology. The last article is devoted to computers, regrettably neglected here.)

Some definitions have a most significant effect. A new concept may be constructed by performing upon objects already at hand operations consistent with their nature. The construction may be quite complicated, applying many established relationships in an arrangement of steps shrewdly chosen to yield a desired result. One example is the introduction in calculus of the definite integral as the limit of a carefully designed expression. Another is the construction of the system of complex numbers from the familiar system of real numbers. The effect of such a definition is to create a new object of discourse. By displaying an accepted symbol or a single brief designation one focuses attention on the object resulting from the construction without having to repeat the construction; the name of the object implies every detail of its generation. The designation "topological linear space," for example, brings to mind a system whose exact definition from first principles, without accompanying examples or comment, would require several paragraphs. Such a definition is valuable insofar as the new object may be brought into illuminating connections with other concepts. Framing such a definition is a creative act.

The words and symbols occurring in the axioms are essential. Further lingo is inessential, but indispensable. The words and symbols occurring in the axioms are *in principle* adequate for the development of the system; abbreviations are not logically essential. But we can't get along without them; both thought and communication would be quite beyond our power. As a supplement to the vernacular, the lingo furnishes unrivalled power and precision. To shrewdly chosen lingo, indeed, the subject owes much of its brilliance.

The lingo is also an invaluable scientific asset, for it is a principal guarantee of that objectivity and general intelligibility absolutely essential to a science. The words *fermé*, *abgeschlossen*, *zamknuti*, *chiusi*, and *closed*, for example, have, in equivalent contexts, technical meanings that are unequivocally identical; confusion would be inexcusable. All this agreement is entirely voluntary, with sanction of no authority stronger than the willingness of the individual to participate in the common enterprise.

As a consequence, however, a mathematician can reasonably address himself professionally only to a rather private audience, which cannot be much larger than that sensitive to esoteric poetry. It is most fortunate, therefore, that his livelihood does not depend on patronage, as a creative individual, by the general public!

The preoccupation of mathematics with concepts, and its dependence on language and symbolism, exhibit strongly the entirely human character of the subject. A. N. Whitehead observes that in mathematics

relationships "are exhibited which, apart from the agency of human reason, are extremely unobvious," and that consequently mathematics "may claim to be the most original creation of the human spirit" (with music a possible exception).

It will come now as no surprise that, in facing the choices which confront him, a mathematician is influenced strongly by aesthetic considerations. An eloquent passage of Bertrand Russell's has justly become celebrated. "Mathematics, rightly viewed, possesses not only truth but supreme beauty . . . a beauty cold and austere, like that of sculpture, without appeal to any part of our weaker nature, without the gorgeous trappings of painting or music, yet sublimely pure, and capable of a stern perfection such as only the greatest art can show." Russell's observer is viewing the finished product. The aesthetic quality of the product results from a pervasive aesthetic concern in the reflections from which it emerged. P. A. M. Dirac has said recently that "it is more important to have beauty in one's equations than to have them fit experiment." To see deeply, with starkest clarity, and to demonstrate perspicuously, with the least means and the most comprehensive revelation of organized pattern, these incentives activate poet, mathematician, and composer.

The criteria of mathematical validity, on the other hand, are entirely public, impersonal, timeless, and devoid of reference to place or human condition. Any mathematical proposition is there for all to see. Once established, moreover, it endures forever.

The accumulating bulk is most formidable. Our children now do glibly calculations that would have baffled Archimedes (though not for long). Older results may be absorbed, of course, into more general newer theories. For this reason, and this circumstance is indeed most fortunate, it is rarely necessary for a beginner to follow the tortuous phylogenetic byways to the present frontiers. We run on paths gouged by earlier workers through rock and thorns.

To round out the picture let us choose only two of the fundamental polarities of human behavior and observe the special form they assume in mathematical activity. The conflict between emotion and reason is a persistent theme in the creative arts. A parallel in mathematics is the delicate balance between intuition and deduction. Each of these faculties is indispensable to the other. It is an over-simplification to say that intuition sees the result and deduction supplies the proof, for the result is not a result until the proof is complete. The aim is simultaneously both to see and to display.

It is true that emphasis has shifted from time to time. Starting with astrology, numerology, and other varieties of mysticism, along with a little hard fact learned from the Babylonians, the Greeks developed that geometry which was the standard of rigorous deduction for two thousand

years. The mathematics of the Age of Enlightenment, by contrast, was logically unenlightened. Tackling quite new problems, mathematicians of that epoch leaped boldly into the unknown, and it is indeed remarkable that unsupported intuition misled so rarely. Rigorous thought has disclosed the pitfalls of that day, and there is now rather widespread agreement on the complementary harmony that should prevail, ideally at least, between imaginative intuition and deductive argument. This harmony has by no means been undisturbed; it has been achieved, in fact, after several crises in which one partner or the other has been neglected. Great difficulties arise when either emotion or reason gains exclusive domination, deaf to the legitimate claims of its companion.

The other polarity we choose to exemplify is that between freedom and discipline. The freedom of a mathematician to devise axiomatic systems is absolute. Once accepted as an object of study, the system imposes on him an extremely severe control. More positively, he must discipline himself to conduct his investigations in rigorous conformity to the specifications of the system. He has accepted quite definite conditions upon the freedom of his imagination and, while giving it every encouragement, he must scrupulously keep it within these limitations. He can go back, of course, and change the system, but that amounts to a renewed exercise of freedom in choosing a system.

In view of the character of a mathematical system, described earlier, the nature and force of the discipline it requires of those who develop it seem fairly clear. What shall we say of the absolute freedom to choose? A system containing an evident contradiction is ordinarily of only pathological interest. But what is interest? A piece of mathematics is interesting if it displays new ideas, or new connections between familiar ideas. Some insight, some imaginative intellectual substance, is essential. This requirement of significance presents a challenge far more profound than that of an isolated puzzle or riddle.

Personal taste comes to the fore. One's sense of intrinsic content depends on the evaluation of related concepts and of the entire context. Usefulness, again, appeals to some more than to others. Elementary arithmetic has very great practical value but negligible mathematical interest. Military and economic affairs confront us with a host of problems. Some seem to be beyond our reach, perhaps still eluding formulation. At the opposite extreme, others have yielded to deft applications of rather simple ideas. While most mathematicians are interested primarily in mathematics for its own sake, many find its practical applications gratifying and illuminating; a very few don't care at all. A mathematician accepts willingly the discipline imposed by the situation in which he is working. His sense of responsibility to himself and to his profession usually guide him toward a wise use of the very great freedom still open to him.

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LIQUID HELIUM*

By LYLE B. BORST

LIQUID helium has amazed physical scientists for more than three decades. As a substance, it is so remarkable that a new category called quantum liquids was established, of which it is the only member. By examining liquid helium, we see a new world, different from the one we know, where quantum phenomena are major effects and are not confined to the submicroscopic world of atomic physics. We perhaps have our first peek into "looking glass house," where things are topsy-turvy. Superfluid helium may be the first material in which the atom plays no part. Superfluid helium may be an element in the Aristotelian sense which is continuous and has no structure.

Helium, as a gas, shows no surprising characteristics inconsistent with its low molecular weight [1, 2, 3]. It will permeate substantial objects, the more rapidly the higher the temperature, but this is true in varying degree for hydrogen, neon and other light gases. Solid helium, likewise, has relatively few characteristics which are unexpected. But liquid helium is a different matter entirely.

The sizes of gas molecules have long been measured by determining the unavailable space during compression. When the free space becomes sufficiently small, the gas condenses to a liquid (if the temperature does not exceed the critical temperature). The size of a molecule of liquid is what one expects from the gas measurements. In liquid helium, however, the density of the liquid is a quarter of that expected, so the molecule (or atom, since helium is monatomic) has four times the expected volume. The wide spacing of the atoms cannot be explained by classical physics and is considered primary evidence of quantum effects.

In 1923, de Broglie asked: If light waves have particle properties, can material particles not have wave properties? de Broglie waves have been the foundation for modern wave mechanics. In the case of helium, de Broglie waves, extending beyond the atoms, overlap and prevent the atoms from approaching each other closely. This wave-overlap prevents liquid helium from freezing. In this respect, helium is unique since it is the only liquid that does not freeze. In order to form crystals, helium atoms must be forced together until the intermolecular Van der Waals forces predominate and form the ordered lattice. Even at temperatures of less than 1°K., solidification occurs only at 30 atmospheres pressure.

Liquid helium at its normal boiling point of 4.2°K. is interesting but not remarkable. It is transparent and colorless, is a poor conductor of

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heat, and is an electrical insulator. It has a low viscosity and a low heat of vaporization. It is a vexing substance to handle but it is not remarkable. As the temperature is lowered, it contracts, as do most substances, until the temperature of 2.18°K. is reached. At this temperature, the liquid suddenly expands. The rate of expansion diminishes as the temperature is further lowered until, near 1°K., the rate is zero and the normal contraction returns. The transition at 2.18°K., is sudden and comprehensive, and marks the boundary between classical and quantum fluids.

The very nature of liquid helium changes at the lambda point (2.18°K.). Although the liquid is a poor conductor of heat above this temperature, below it, it becomes the best conductor we know—500 times better than coppr or silver! Generally, good conductors of heat are also good conductors of electricity, but helium has no free electrons and remains an electrical insulator. Thermal conductivity in helium must be of a completely new variety; for its description, a new phenomenon is identified: second sound. Above the lambda point, liquid helium is constantly boiling, since even in the best apparatus some heat penetrates. Below the lambda point, all bubbling stops, for the heat is conducted to the free surface where evaporation takes place. This transport of heat is best described by the equations of acoustics; and although no one will ever hear "second sound," the term is appropriate.

Above the lambda point, liquid helium can be stored indefinitely in a Pyrex vacuum flask. Below the lambda point, however, the vacuum soon deteriorates because helium creeps through the glass. Helium-II, the low temperature form, is a superfluid. It penetrates cracks and holes that no other material can find. The unwary apparatus designer who leaves a void in his apparatus accessible by superleak, will have an explosion when the temperature rises, for pressures of thousands of atmospheres can easily develop.

Attempts to measure the viscosity of the superfluid lead to contradictory values. Andronikashvili cleverly demonstrated these anomalies by studying the rotation of a series of closely spaced discs immersed in the liquid. His apparatus consisted of a verticle shaft upon which the discs were mounted supported by a torsion fiber, so that the system could oscillate much as the balance wheel of a watch. He noted the period of oscillation in air and, knowing the characteristics of his fiber, determined the moment of inertia of his system. If the same measurement were made in a viscous liquid (like salad oil) the moment of inertia would be that of the shaft, the discs, and the oil between discs. In helium above the lambda point, the liquid was carried by the discs and showed the expected density. At a temperature near 1°K., however, no helium was carried. Apparently the liquid showed zero viscosity, for it did not reflect the motion of the nearby discs. As the temper-

ature was raised, various fractional densities were observed until the full density was reached at the lambda point. The term "normal component" was applied to the fraction of the liquid which rotated, whereas the remainder was termed the superfluid component.

If two branches of a U tube be connected by a capillary and if one branch be filled higher than the other, the liquid levels will oscillate over minutes or hours until they become equal. No such phenomenon is known except in liquid helium below the lambda point. The explanation given is that the normal component cannot pass the capillary and that the superfluid passes back and forth, changing the composition and the temperature. If, then, one branch is heated, the composition of the fluid will change and the superfluid will move toward the high temperature side, producing a pump. This appears to be a violation of natural laws akin to perpetual motion. When, however, the applied heat is included in the thermodynamics, the system becomes a heat engine, without working parts, in which heat is converted directly to mechanical work. Now if the hot branch has a restriction, hydraulic pressure is produced and liquid is projected to form a fountain when the liquid reaches this point. This fountain continues to play as long as there is helium in the system and the two temperatures are below the lambda point and are unequal.

When an open-topped container filled with liquid helium is suspended above a pool of liquid (always below 2.18°K.) it will slowly empty itself. This emptying is not due to diffusion through the wall. This happens in any shape container made of any material. The liquid wets any solid, creeps up the wall, and overflows to drip off the bottom. Again, this is a quantum phenomenon not found in other substances. The "velocity of creep" is nearly independent of the nature of the container and its height, the rate of flow depending primarily on the wetted perimeter.

In contrast to helium-4, the rare isotope, helium-3, shows none of these remarkable superfluid characteristics. Helium-3 was first studied in order to throw light on helium-4; however, it soon showed such remarkable behavior that it is now studied in its own right. In helium-3, there is an anomaly in thermal expansion but of a very different variety. Helium-3 is paramagnetic. Although helium-4 and helium-3 are isotopes, they are more unlike than any other pair of isotopes. In fact, they are so different that, when a solution of equal parts is cooled below 0.9°K., the liquid separates into two phases. The heavier, the helium-4-rich phase, shows superfluid properties, somewhat suppressed. The lighter helium-3 phase shows no superfluidity. Again these differences are of a quantum nature. Helium-4 has even particles in its nucleus, two protons and two neutrons whereas helium-3 has odd, two protons and one neutron. Odd and even numbers in quantum mechanics are given the

most profound importance. The statistics first proposed by Bose and Einstein apply to light and generally to systems having even characteristics. Fermi and Dirac developed statistics particularly appropriate to the electron and to systems with odd characteristics. Helium-4 uses the mathematics appropriate to radiation problems and helium-3, the mathematics of electric systems.

Quantum mechanics is usually considered as a highly mathematical branch of theoretical physics divorced from everyday experience. Historically, this has certainly been true; still, there are quantum problems in our world which have the salient quantum features and which differ primarily in scale from atomic systems. As an illustration, let us take the problem of traffic in a city. In order to simplify the problem we shall assume the blocks to be square and of identical size. A common size is eight blocks to the mile. Now let us assume that there is a traffic light at every intersection. All lights turn green in a north-south direction simultaneously and 30 sec later turn green east and west. At what speed can cars travel in any direction without being stopped by a red light? If a man driving north covers a block in just 1 min, lights will turn green as he approaches, however far he drives. His velocity will be:

$$v = d/t = \tfrac{1}{8} \text{ mile per minute} = 7.5 \text{ mph}$$

South-bound cars can travel at this speed also and so can cars traveling east and west!

Is this the only solution to the problem? A car driving faster than 7.5 mph will inevitably be stopped by a red light. If it travels at half this speed, it will cover a block in 2 min. At one-third the speed, in 3 min, etc. So the complete solution is

$$v = \frac{7.5}{n} \text{ mph}$$

with n an integer. The quantum number n is a consequence of the uniformity of the street grid. Quantum numbers in atomic problems appear in like manner whenever periodicity occurs.

Such quantum numbers also occur in acoustics. In the case of the violin string they are called harmonics. If the string vibrates as a whole then $n = 1$ and the tone will be the fundamental. Another tone an octave higher can be obtained by damping the string at its midpoint. The string vibrates in the form of an S with stationary midpoint and $n = 2$. The third harmonic is an octave and a half above the fundamental. In this case the string vibrates in three sections. So, n can therefore take any integral value. In the case of a string of beads, however, n can take values only to the number of the beads, N, for the Nth harmonic will represent adjacent beads oscillating in opposite directions.

The timpani or kettle drum is a two dimensional vibrating membrane

which shows similar quantized overtones involving two independent quantum numbers.

Vibrations of a three dimensional solid require three quantum numbers n_x, n_y, n_z, of exactly the same nature. The only musical instrument based upon vibrations of a massive solid is the musical anvil—a most limited instrument. In the atomic realm, however, the theory of acoustic vibrations of a crystalline solid gave rise to a scientific achievement which will stand through the centuries. In 1912 Debye studied this problem by counting the number of vibrational modes or harmonics as a function of the quantum energy of the vibration. He obtained an expression for the internal energy and heat capacity which has been the cornerstone of our understanding of the solid state. He determined that the heat required to raise the temperature of a given mass of solid one degree must vary as the cube of the temperature, if the temperature is sufficiently low. As the temperature rose, higher values of n occurred so that as the quantum number n approached the number of atoms N, as in the case of the beads on the string, no new vibrations were possible and the theory became that of a classical substance described by the law of Dulong and Petit.

Liquid helium shows the characteristics of a Debye substance below 0.7°K. and above 3°K. Between these temperatures we have the tremendous anomaly in heat capacity and thermal expansion. The normal component of Andronikashvili correlates perfectly with the internal energy of the anomaly. The nature of the superfluid, the nature of the anomaly, and the nature of the lambda transition are the perplexing phenomena that a satisfactory theory must explain.

Current Theories

Many theories have been proposed, differing in their starting point and emphasis. No current theory gives a satisfactory explanation of all phenomena so that the problem is by no means solved.

The first general theory was that of Landau, first proposed in 1941 and modified in 1947 [4]. He considered the rotation of the whole liquid to be a quantum phenomenon and derived a relation between the energy and momentum of these quantum states or excitations. He named his excitations rotons in contrast to the acoustical excitations called phonons.[1]

In Figure 1, energy is plotted vertically and momentum horizontally. The rate of change of energy with momentum measures a velocity, so the slope of the curve represents the velocity of sound. Phonons have a velocity independent of their energy so that they are represented by the

[1] In quantum mechanics waves and particles are equivalent so for each particle there must be a wave and for each wave there must be a particle, e.g., in electromagnetic radiation for each light wave there is the particle called the photon; in acoustics for each sound wave there is the particle called the phonon.

straight line through the origin. Landau's rotons show a parabolic relationship and are represented by the dotted curve.

This assumed excitation permitted Landau to fit the experimental heat capacity to 1.6°K., using three empirically determined constants. The same three constants were used to fit the normal component as well. Landau and other workers have adapted the theory to a wide variety of phenomena involving the superfluid. It has been a most constructive and useful theory and has been the basis for most experimental investi-

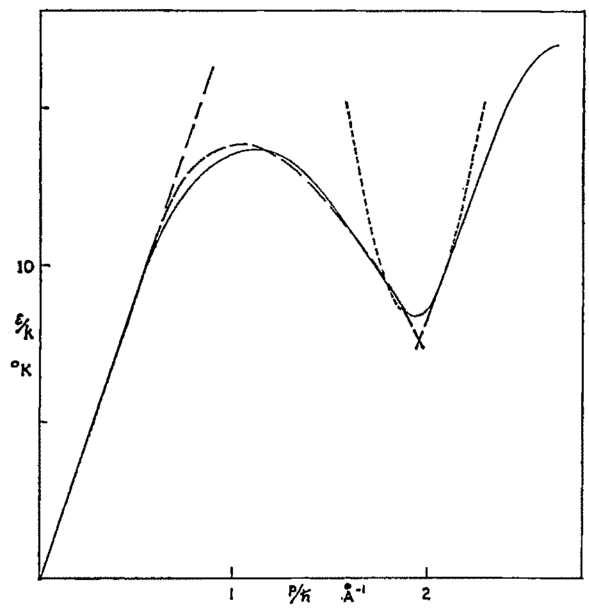

Fig. 1. Dispersion relation between the energy of an excitation and the momentum. Dotted line, Landau roton; continuous line, experimental results of neutron scattering; dashed line, proposed theory.

gations. Generally speaking, the superfluid component has a negative definition. It is the absence of rotons, i.e., the absence of the normal component. Below 0.7°K., liquid helium shows no excitations other than phonons and can be described by the Debye theory. This is also true of solid helium and of liquid helium near its normal boiling point of 4.2°K. where there is no evidence of superfluidity. Landau gives no physical interpretation of the lambda point except as the temperature at which the normal component reaches 100% (and the superfluid concentration becomes zero).

A more recent interpretation of superfluidity has been given by Feynman [5]. He points out that the excitations are of two kinds, one of long wave length involving the coordinated, coherent oscillations of

large numbers of atoms (phonons), the other of higher energy and shorter wave length involving the motions of a small number of atoms (rotons). He considers the ultimate theory derivable from the proper Schroedinger wave equation. In the absence of this equation, he tries various wave functions and chooses that representing a vortex or smoke ring as the model having properties most like rotons. He obtains a dispersion curve (the relation of energy to momentum) similar to that found by neutron scattering. He associates the superfluid with the absence of molecular size excitations, vortices or rotons. Again there is no physical model of the lambda transition.

Cell Theories

A new theory to justify consideration must either (*a*) give a rigorous relationship between observations and a set of well defined postulates or (*b*) explain and relate observed phenomena in a new light, encompassing a wider range of observations and showing new relationships. The present proposal cannot yet be derived from the Schroedinger equation, although this is anticipated; nevertheless, it does interpret experimental observations quantitatively in a totally new light.*

de Boer [6] and Temperley [7] have independently proposed cell theories of liquid helium. Atoms are considered in pairs, free to rotate within a sphere defined by their neighbors. The two kinds of excitations are then the coordinated motions of the centers of gravity of many cells—the acoustic modes or phonons, and the rotational states of the diatomic rotors within the cell. The Schroedinger equation can be expressed and solved subject to the adiabatic approximation, i.e., that rotation does not affect vibration and acoustic vibrational modes are not coupled to the rotations within the cell. The results are the vibrational modes of Debye theory and a set of equally spaced rotational states of the diatomic rotor. This theory gives a clear differentiation between helium-3 and helium-4, since helium-3, having an odd number of particles in the nucleus, will show all rotational quantum numbers whereas helium-4 will show only even values ($J = 0, 2, 4$). The absence of rotation ($J = 0$) will be the lowest state where only phonons exist and will be associated with the Debye liquid for $T \leq 0.7°K$. Experimentally, the odd-even relationship accounts roughly for the fact that anomalies in helium-4 occur in the 1–2°K. temperature range whereas in helium-3 they occur at 0–0.3°K. A critical examination of experimental data does not, however, disclose evidence of well-defined rotational levels. The weak coupling (adiabatic approximation) between rotation and vibration would seem to be in question.

The present proposal is a strong coupling theory in which vibrations and rotations are thoroughly and indistinguishably mixed. Since an

* Note added in proof: A derivation from the Schroedinger equation has now been completed along lines somewhat different from those of the appendix. It will be published shortly.

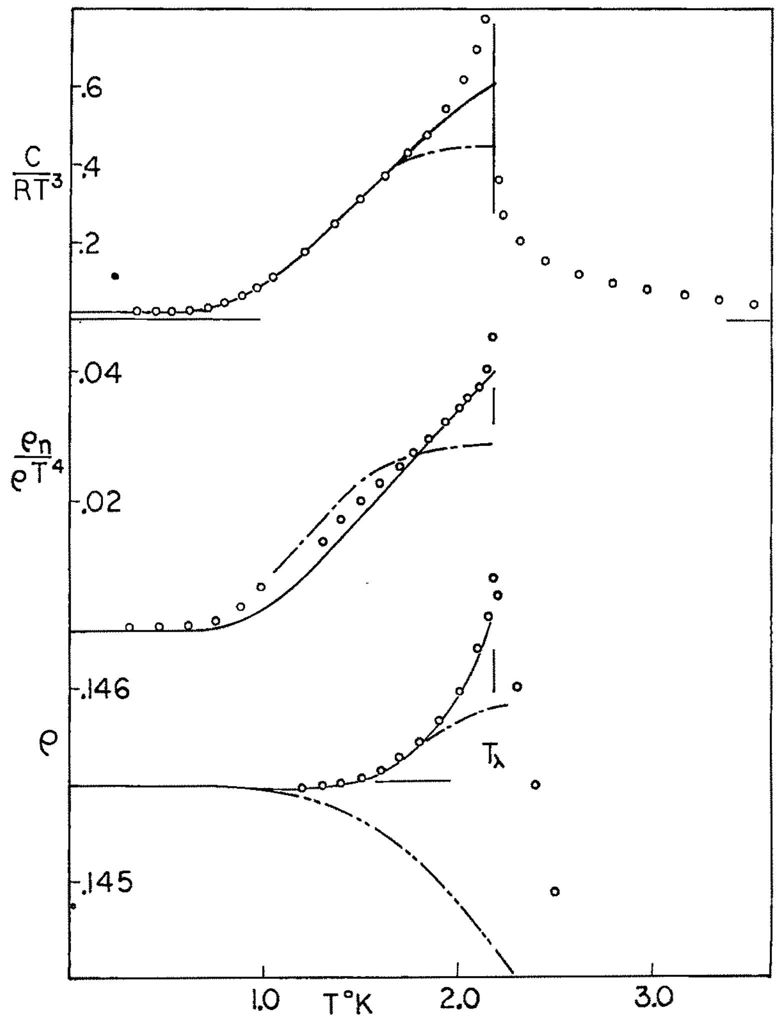

FIG. 2. Temperature dependence of thermodynamic variables. Circles, experimental data; continuous line, proposed theory; dot-dash, Landau theory; dash-double-dot, Debye theory. Top, heat capacity; center, normal component; bottom, density.

analytical derivation from the Schroedinger equation has not yet been obtained, an approach is taken as an extension of Debye theory. In Debye's theory, the modes in the x, y, and z directions are thoroughly and completely mixed or coupled by assuming arbitrary and unrelated values of n_x, n_y, and n_z. A vibrational mode is described by any three values of n. The T^3 heat capacity results from this assumption. Roughly speaking, the experimental heat capacity of helium below 2°K. varies as the fourth power of the temperature. A four dimensional Debye

theory therefore gives rough agreement with the data. If now the fourth dimension is related to the rotational mode of the diatomic pair, a non-homogeneous space is defined which acts like three dimensions at low temperature (where $J = 0$) but four dimensions at higher temperatures (where $J = 2$ is populated). Complete representation of the heat capacity and of the normal component can be achieved to about 2°K. well beyond the region of validity of the simple Landau theory (Fig. 2).

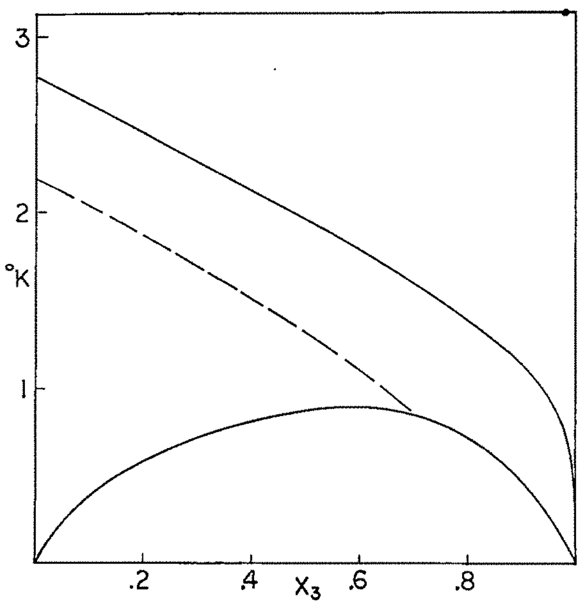

FIG. 3. Phase diagram helium-3 helium-4 solutions. Atomic fraction, helium-3 plotted horizontally. Upper line, calculated lambda temperature; dashed line, observed lambda temperature. Lower line, phase separation boundary.

The density of the liquid can be fit to 2.12°K. if an additional assumption is made that the $J = 2$ state has a density 1.2% greater than the Debye liquid[2].

The range of validity of the Debye theory is limited to temperatures for which the number of acoustical modes (phonons) does not exceed the number of atoms or molecules. The present theory gives a similar set of countable states (involving rotation as well as vibration). When one equates the number of modes to the number of molecules, a temperature of 2.7°K. is obtained which represents the upper bound of the theory. It

[2] In the case of liquid hydrogen, the high temperature form (75% ortho, 25% para) has a density 0.5% greater than the pure para ($J = 0$) form. Thermal expansion will therefore not vary as T^3 but will reflect the ortho-para composition at equilibrium at each temperature.

is believed that 2.7°K. is the theoretical value of the lambda temperature (2.18°K. by measurement). The theory cannot be valid above this temperature.

The proposal requires the consideration of pairs of identical helium atoms. No permanent binding is necessary since only quantized rotation is required, the nearest neighbors defining the cell. The nearest neighbor distance is therefore assumed for the diatomic rotor. If helium-3 is added to the liquid, He^3-He^4 pairs form, thereby reducing the number of He^4-He^4 pairs. If the number of modes is now counted to the number of He^4-He^4 pairs expected, the lambda temperature is found as a function of helium-3 concentration. A comparison with experiment (Fig. 3) shows the form and the slope to be correct but, since the theory predicts 2.7°K. for pure helium-4, the lambda temperatures are uniformly high. Support for the consideration of atoms in pairs results also from a study of the Andronikashvili experiment in He^3-He^4 solutions. The concentration of the superfluid component extrapolated to zero degrees will be 100% for pure helium-4. In solution, these extrapolated values diminish twice as fast as would be expected for monatomic constituents, but quite in accord with the assumption of paired atoms.

There is at present no adequate explanation of the lambda point. It is considered to be a "second order" phase transition, and is virtually unique among liquids. In solids, similar discontinuities are common and are associated with a change in the crystalline structure. Since liquids have no crystalline lattice, this explanation is not permissible. A transition of the same nature was observed in liquid sulfur at 160°K. during the nineteenth century (Fig. 4). Here, the molecular structure has been well characterized both above and below the transition temperature. The transformation is one of polymerization from an eight membered ring (the lambda form $T <$ 160°C.) to a linear polymer of large molecular weight (the mu form $T >$ 160°C.). The heat capacity becomes large and has the same character as liquid helium although reversed in the sense of temperature. By analogy, we can say that the liquid becomes increasingly ordered as the transition temperature is approached, in sulfur by increased molecular weight of the polymer; in helium by increased number of rotation-vibration coupled modes. The ordered state becomes thermodynamically unstable and transforms into the simpler form: in sulfur the eight-membered ring; in helium the monatomic liquid.

The lambda point is therefore identified as a dissociation temperature for the paired states. This occurs near the temperature at which the number of modes equals the number of molecules. Above this temperature the liquid is monatomic, again showing the character of a Debye liquid, but lacking the character of a quantum liquid. There appears to be a dissociation energy of 1.2 calories per gram—one-fifth the normal heat of vaporization.

The remarkable achievements of Landau's theory stem from an assumed relation between the energy and momentum of his excited (roton) states. A major accomplishment, predicted by Feynman, was the measurement of this relation by the inelastic scattering of neutrons. An examination of Figure 1 shows why Landau's assumption was constructive,

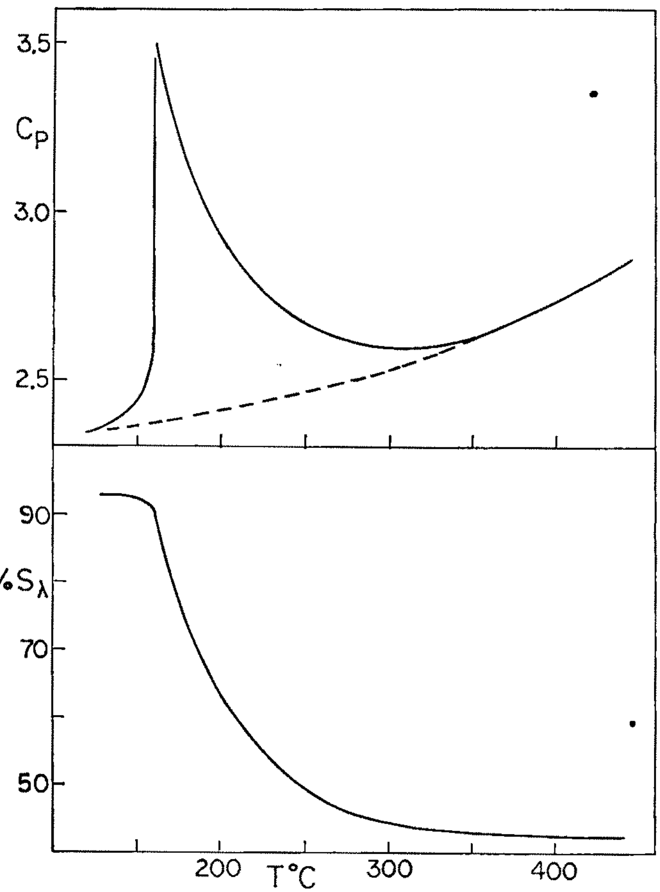

FIG. 4. Phase transition in liquid sulfur. Above, heat capacity; below, composition, per cent low temperature form (lambda).

for the states near the minimum are those which have roton character. Deviations from the low energy, low momentum phonon states are not a part of Landau's theory, but are an important aspect of the present proposal.

It is therefore important to derive the dispersion relation from the helion (strong coupled cell) theory. This is done by assuming that both the roton and helion excitations give alternative and correct expressions

for the internal energy. These expressions are equated and common factors are cancelled to give a relation between the excitations of classical statistical mechanics and the helion. A fixed value of Debye's relative coordinate ($x = h\nu/kT$) is introduced and the resulting expression gives an excellent fit of the observed curve up to the minimum. In this formulation, the minimum corresponds to the lambda point at which the number of modes equals the number of molecules and dissociation occurs. Beyond this minimum only phonons will exist—a reasonable interpretation of the empirical data.

The theory suggests that the minimum of the dispersion curve is another expression of the dissociation occurring at the lambda temperature. If this be true, then the minimum of the dispersion curve should follow the lambda temperature as a function of applied external pressure and in He^3-He^4 solution.

Interpretation

Liquid helium is a Debye liquid at the lowest temperatures. As the temperature is raised, acoustical modes are excited, giving rise to the T^3 dependence of the heat capacity. The de Broglie wave length is long compared to any characteristic dimension, and the theory can be considered that of a continuum. As the temperature approaches 1°K., the de Broglie wave length is approaching atomic dimensions. The longest constructive dimension in the system is the circumference of the orbit of rotation of an atomic pair (5.8 Å). Since the nearest neighbor distance (3.7 Å) is large compared to the size of the atom (the effective diameter of the electron cloud of the helium atom is 2.7 Å) and no well-defined lattice exists in the liquid, the diatomic pair can be considered enclosed in a spherical potential defined by the nearest neighbors. Quantized free rotation will then be excited after the manner of the rigid rotor. The first rotational state occurs at 5.1°K., comparable to the energy of the vibrational states (phonons) and cannot exist at equilibrium in the absence of phonons. The rotational and vibrational states are therefore assumed to be strongly coupled. As the temperature approaches T_λ, more and more pairs are excited until all atoms are participating. The number of modes then equals the number of molecules. The addition of further energy causes dissociation to the monatomic liquid, reverting to the Debye liquid with conventional heat capacity and expansion.

The superfluid appears to be a state of complete quantum-mechanical repose. At the absolute zero of temperature, no vibration nor rotation occurs. At finite temperature, the superfluid is that fraction of the liquid associated with neither acoustic nor rotation-vibration modes. The de Broglie wave length is long compared to any atomic dimension (15 Å at 1°K.) and the liquid can be treated as a continuum. In this state, one can question the utility of the atomic theory, since the liquid appears to be structureless. It can penetrate openings of less than atomic dimensions. The flow of the liquid as a film may be considered as a consequence

of this structureless character. An atomic liquid will have a boundary at which the density of atoms suddenly changes or becomes zero. If the liquid is structureless, a boundary is a paradox. At the edge defined by liquid, solid, and vapor, the liquid would appear to respond by creeping over the solid and the paradox of the edge of a continuum will be avoided. The Andronikashvili experiment is understandable in terms of the continuum, for viscosity is explained by a finite free path which carries atoms into the flowing fluid. If, however, no atoms are present in the continuum, no mean free path is possible and no momentum can be transferred.

This theory of excited states would seem to be general for monatomic noble gases, limited only by the freedom of the paired atoms to rotate in the cell of their neighbors. Helium-6, a radioisotope of 0.8 sec half life, would surely form a superfluid if prepared in high concentration. An estimate indicates that the lambda point of the pure material would be expected near 1.5°K. The next heavier noble gas is neon of atomic weight 20. The de Broglie wave length would be less than half that of helium at a given temperature and the quantum mechanical wave interference would be expected to be considerably less. The combination of smaller nearest neighbor distance and greater atomic weight gives a rotational constant one-quarter that of helium, so that the Debye fluid, if it forms, would be found only below 0.2°K. The existence of a lambda point cannot be predicted with assurance since the rotational state falls below the temperature at which Debye modes are numerous and the requirement for strong coupling may not pertain. Moreover, the theory is that of a liquid with no long range order, so liquid neon would have to be supercooled below its normal freezing point of 25°K. If, however, this experimental difficulty can be overcome, superfluid characteristics might well be observed at temperatures of a few tenths of a degree.

Conclusion

A new theory is proposed as an extension of the Debye theory in which pairs of atoms are assumed to rotate and these rotational excitations are strongly coupled to the acoustical vibrations of the Debye theory. The rotation-vibration excitations, called helions, are analogous to the rotons of Landau and the vortices of Feynman. They account quantitatively for the thermodynamic anomalies in liquid helium, predict a lambda point, account for the lambda point in He^3-He^4 solutions, and give a dispersion relation of the observed form. The superfluid is that fraction of the liquid corresponding to neither acoustic nor vibration-rotation excitations, or that fraction in the zero momentum or ground state. As such, the superfluid is a continuum and its characteristics imply the absence of discrete atoms.

The support of the National Science Foundation during the early part of this study is gratefully acknowledged.

Appendix

Strong coupling is assumed because the assumptions of the adiabatic approximation are not fulfilled. The acoustical velocity is 240 meters per second as compared to the velocity of helium atoms in the $J = 2$ rotational state in a 3.7 Å diameter orbit of 170 meters per second. These velocities do not differ sufficiently to justify weak coupling.

In the present theory, strong coupling is achieved by considering an inhomogeneous space of four dimensions in which the three equivalent axes are Debye coordinates but the fourth is associated with rotation of atoms in pairs. A four dimensional spheroid has the formula:

$$\frac{W^2}{a^2} + \frac{X^2}{b^2} + \frac{Y^2}{b^2} + \frac{Z^2}{b^2} = 1$$

The volume of the spheroid is

$$v = \frac{\pi^2}{2} b^3 a$$

and the differential volume with respect to b is

$$dv = \frac{3}{2}\,\pi^2 a b^2 db$$

Debye theory gives $b = 2\,\nu/c$ where c is the velocity of sound, and ν is the frequency of the acoustical mode. The number of cells available becomes

$$dZ = \frac{12\pi^2 a}{c^3}\,\nu^2 d\nu$$

and the internal energy per mole is

$$dU = \frac{Vh\nu dZ}{e^{h\nu/kT} - 1}$$

where V is the molecular volume, h, Planck's constant, k, Boltzman's constant and T the absolute temperature.

The expression for a is taken from the energy of rotation of a diatomic gas and is the fraction of the molecules in rotation multiplied by an integer presently assumed to be $J(J + 1)$

$$a = \sum_{J=2,4,\cdots} \frac{(2J + 1)e^{-J(J+1)B/kT}}{\sum\limits_{J=0,2,4,\cdots} (2J + 1)e^{-J(J-1)B/kT}}\, J(J + 1)$$

B is the rotational constant $h^2/8\,\pi^2 I$ with I the moment of inertia. J is the rotational quantum number which for homonuclear atoms with zero nuclear spin is confined to 0, 2, 4,. . . . In nearly every instance, contributions from $J = 4$ and larger are trivial so a may be approximated by

$$a = \frac{30e^{-6B/kT}}{1 + 5e^{-6B/kT}}$$

The low temperature approximation of Debye may be assumed since T_λ is one-tenth the Debye temperature. The internal energy and heat capacity of the helion anomaly then become

$$U = \frac{24\pi^6 V k^4 T^4}{c^3 h^3} \frac{e^{-6B/kT}}{1 + 5e^{-6B/kT}}$$

$$C = \frac{48\pi^6 V k^4 T^3}{c^3 h^3} \left[\frac{2e^{-6B/kT}}{1 + 5e^{-6B/kT}} + \frac{3Be^{-6B/kT}}{kT(1 + 5e^{-6B/kT})^2}\right]$$

To this helion heat capacity must be added the Debye (phonon) contribution which will predominate below 0.7°K. but which is not significant above 1°K.

Empirical constants entering the expression are the molecular volume (55 cm³/8 gm mole), the velocity of sound (237 m/sec) and the nearest neighbor distance (3.7 Å) which enters the moment of inertia of the rotor.

The normal component is accurately correlated with the internal energy. Landau derived an expression for the density of the normal component $\rho_n = 4U/3c^2$ where c is the velocity of propagation of the excitation: for phonons, the velocity of first sound; for helions, the velocity of second sound (18.5 m/sec).

The number of helions may be calculated

$$dN = \frac{V}{e^{h\nu/kT} - 1} dZ$$

If the low temperature approximation is used, the integrated expression becomes

$$N = \frac{2.404(360) V k^3 T^3}{c^3 h^3} \frac{e^{-6B/kT}}{1 + 5e^{-6B/kT}}$$

If N is Avagadro's number, a temperature of 2.7°K. is obtained analogous to the Debye temperature (20°K.). This is identified with the lambda point. In a solution of helium-3 in helium-4 only He^4-He^4 pairs can show the postulated quantized rotation. If pairs occur statistically, the concentration will be proportional to the square of the mole fraction $N_{44} = X_4^2 N$. The calculation of N_{44} gives a curve of T_λ as a function of mole fraction. (In this calculation $J = 4$ may be detected for nearly pure helium-4.)

The density of the liquid can be calculated by assuming a perfect solution of two components, the Debye liquid and the roton or helion component, differing in intrinsic density by 1.2% but having the same expansion coefficient ($\alpha = 7.5 + 10^{-4}\, T^3$).

To obtain the dispersion relation, the expression for the internal energy of this theory is equated to the classical expression

$$\frac{4\pi V}{h^3} \int \epsilon e^{-\epsilon/kT} p^2 dp = \frac{4\pi V}{h^3} \int \frac{(1 + 3\pi a)h\nu}{e^{h\nu/kT} - 1} p^2 dp$$

Since the limits of integration are identical, the integrands may be equated in the approximation of Boltzman statistics,

$$\epsilon e^{-\epsilon/kT} = (1 + 3\pi a)h\nu e^{-h\nu/kT}$$

Debye's relative coordinate $x = h\nu/kT$ is introduced together with an additional adjustable constant alpha

$$\epsilon e^{-\epsilon\alpha x/h} = \left(1 + \frac{90\pi e^{-6Bx/h}}{1 + 5e^{-6Bx/h}}\right)h\nu e^{-\alpha x}$$

If x is assumed constant, the frequency ν is formally analogous to the temperature in the earlier expressions. Since frequency is proportional to momentum, momentum states will contribute in sequence to the energy. Again these momentum states may be counted to Àvagadro's number so that the lambda temperature should have an equivalent momentum. The minimum of the dispersion curve at 1.95 Å^{-1} is therefore identified with the lambda point at 2.18°K. This permits the evaluation of the constant x as 16.5, a value which gives a good fit where the curve departs from the phonon line. The theoretical fit in Figure 1 is achieved with x = 18 and α = 0.3. No *a priori* justification can be given for this value of alpha at this time.

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A DECADE OF MOTIVATION THEORY

By D. E. BERLYNE

A CONSPICUOUS part of contemporary psychology consists of what is coming to be known as "motivation theory." This term does not denote a coherent system of axioms and theorems, as "set theory" and "special relativity theory" do, or even to the extent that at least parts of "learning theory" do. Some psychologists regard motivation theory as indissociable from learning theory and place them both within "behavior theory." Others would be more inclined to annex motivation theory to "personality theory." Motivation theory consists of an area of inquiry, and, although it would be hard to obtain agreement on where precisely its boundaries lie, its territory is demarcated by questions rather than by answers.

For some time after the launching of psychology as a scientific discipline, experimental psychologists did their best to manage without motivational concepts and did not feel their lack. The earliest pioneers were interested largely in eliciting descriptions of the conscious experiences produced in human subjects whose sense organs were being exposed to particular kinds of stimuli or who were engaged in particular kinds of tasks. Motivational problems arise in these situations as elsewhere, but they were masked by the ease with which verbal instructions or requests can generate artificial motives in human subjects, especially when they are colleagues or graduate students of the experimenter, as was often the case in those days. A little later, men like Watson, Thorndike, and Pavlov began to seek laws governing behavior, beginning with the simplest forms which could most conveniently be studied in animals. They likewise felt no need to consider the motivational condition of the organism, being content to analyze behavior in terms of "reflexes" or stimulus-response "connections," which meant relating muscular and glandular reactions to events in the external environment. Two of their contemporaries, Freud and McDougall, having become involved in abnormal psychology and in social psychology respectively, were brought face to face with some of the dramatic extremes reached by behavior outside the laboratory and felt compelled to denounce the vanity of attempting to explain animal or human actions without reference to "urges," "instincts," "purposes," "goals," etc.

Psychologists primarily interested in individual differences and personality were readily converted to this way of thinking. Experimental psychologists were more resistant, but, by the beginning of the 1930's, which is the time when the early behaviorism of Watson gave place to the various brands of neo-behaviorism, they too had absorbed the lesson.

The contentious concept of "instinct" had been replaced by the less provocative concept of "drive," a term apparently first taken over from mechanical engineering but later used in the plural under the influence of the German word "*Triebe*" (the word that Freud's translators rendered as "instincts"). Various techniques for measuring the strength of drive (or drives) experimentally had been introduced, and several experimenters had demonstrated that even the simplest forms of animal learning are profoundly affected by the motivational condition (drive level) of the subject. Theoreticians like Holt, Tolman, and Hull had devised ways in which the purposive, goal-striving nature of behavior could be acknowledged and yet treated in full conformity with the canons of scientific discourse. They studied the effects of motive and drive level as factors that fluctuate from moment to moment in the life-history of one individual. Personality theorists were more concerned with "motives" or "needs" as persistent factors varying from individual to individual.

To attack motivational problems means to seek factors that govern the organism's degree of alertness and activation, that bias the organism towards certain forms of behavior, and that determine what events will provide reinforcement for learning processes and how effectively. This might seem to involve three distinct groups of problems rather than one psychological topic. Nevertheless, the three groups appear to be interrelated and are commonly treated together, because the best known drive states, e.g., hunger and fear, are apparently conditions that activate animals as well as conditions that bring certain forms of behavior to the fore (e.g., eating, being startled, moving in directions where food or safety has been found in the past) and "aversive" conditions whose termination is rewarding or reinforcing.

Since 1952, theoretical and experimental work on motivation has been carried on under watchful surveillance from Lincoln, Nebraska. The Nebraska Symposium on Motivation has annually summoned some five or six psychologists and near-psychologists to deliver themselves of their findings and ideas. This does not mean that the organizers of the Symposium have mustered 60 specialists in motivation. There are, actually, surprisingly few people who would accept that label. Many of the participants must have been quite surprised to find themselves associated with motivation, and a few were put to evident strain to find some connection between what they had been doing and motivational problems. Nevertheless, all psychological processes presumably have motivational aspects, and all the contributors had the experience, which must have been salutary for them and for their readers, of being forced to consider their areas of interest from this angle, which they might not have done otherwise.

The first decade of the Nebraska Symposium has been a singularly

eventful one for motivation theory. First, there have been momentous neurophysiological advances whose psychological implications must take some time to work out and which show no prospects of slackening their pace. The discovery by Moruzzi and Magoun, reported in 1949, that the reticular formation of the brain stem governs the electroencephalographic patterns indicative of an alert brain gave rise, when put together with the proceeds of other lines of research, to the psychophysiological concept of "arousal level," a development whose potential consequences are far-reaching. The newer concept of "arousal" has obviously much in common with the older concept of "drive," especially in its activation aspect. We know of many conditions that will raise arousal in addition to the familiar conditions that have long been known to raise drive level, and we have at our disposal several convenient, relatively direct ways of measuring arousal to supplement the indirect, cumbersome techniques that have had to be used hitherto to measure drive.

Neurophysiological discoveries relating to response selection and reinforcement have also not been lacking. Stimulation and ablation experiments have identified numerous points in the lower parts of the brain—in the hypothalamus, thalamus, and limbic system—whose activity brings to the fore, or inhibits, behavior appropriate to specific motivational conditions, e.g., feeding, drinking, sleeping, sexual behavior, aggressive behavior, and flight. Since 1954, when Olds and Milner and Miller, Delgado, and Roberts reported finding structures in the lower cerebrum and brain-stem whose stimulation had effects resembling those of externally applied rewards and punishments, the study of the functioning and inter-relations of these structures has steadily intensified and proved extremely productive.

On a more purely psychological level, the last ten years has seen rapid growth of interest in exploratory behavior and related phenomena. It came to be realized that, in higher animals, a great deal of time is spent on the multifarious activities that are customarily placed under such headings as "curiosity" and "play" and bear no obvious and immediate relation to familiar, "organic" motives. Experimental work has revealed that the likelihood of these activities and the directions that they take depend largely on factors like novelty, surprisingness, and complexity of external stimulation. These factors (which are closely related to the concepts of information theory) seem, in their turn, to be capable of generating the kind of disturbance that motivates behavior and promotes acquisition of new learned responses, apparently because they involve the simultaneous initiation of discrepant or mutually interfering reactions.

This means that we must add disharmonious relations among neural processes to the visceral upheavals (e.g., hunger, fear) and the external irritations (e.g., pain, excessive cold) that have long been recognized as

sources of drive. We must also add a large range of new rewarding conditions, working through relief of conflict, to the rewarding events that can reinforce learned responses. The new lines of thought that have thus been opened up by work on exploratory behavior are converging in significant ways with ideas that have emerged from recent work on attitude change, educational practice, child development, personality, aesthetics, and humor. Factors like novelty, complexity, and conflict now appear to play a prominent and previously overlooked role in all these areas.

Further impetus for rethinking has come from the ethological movement, which was launched by continental European zoologists interested in animal behavior, particularly in that of insects and lower vertebrates. Ethology was introduced to psychologists by the publication of Tinbergen's book, *The Study of Instinct*, in 1951. In consequence of the interchanges between ethologists and psychologists that resulted, some of the theoretical statements of the former have been criticized and refurbished. On the other hand, both animal and human psychology have appreciatively received a mass of new experimental problems, new experimental techniques, and new theoretical notions. The ethologists made clear the prevalence of patterns of "instinctive" or "species-specific" behavior in lower animals, stemming from inherited properties of the nervous system and yet much more complicated and flexible than the reflexes and chains of reflexes that were formerly thought to comprise inherited behavior patterns. Above all, these forms of behavior are profoundly affected by the animal's motivational condition. So, while the ethologists have raised the possibility that the kinds of instinctive behavior that they have brought to light may be present in higher animals, including human beings, their main contribution has perhaps been to indicate the close interpenetration of unlearned and learned elements in behavior and the susceptibility of both of them to common principles, especially common motivational principles.

Volume XI in the Nebraska series* shows a polarization reflecting the tension between old and new trends in current motivational theory. Rogers and Sears, in their articles on "actualizing tendency" and "dependency motivation," are concerned with motivational aspects of personality and extend inquiries that they have been pursuing for some years. The three remaining contributors, N. E. Miller, Pribram, and Magoun, discuss long-standing problems pertaining to reinforcement and to inhibition in the light of recent neurophysiological advances. Miller's paper is particularly interesting to anybody who likes to spot trends. Having been one of the most diehard and ingenious defenders of the view that reinforcement depends on drive-reduction, Miller here outlines an

* M. R. Jones (Ed.) *Nebraska Symposium on Motivation 1963*, Lincoln, Nebraska: University of Nebraska Press, 1963.

alternative, and in some ways diametrically opposed, view that brain-stimulation experiments have forced him to consider. He now speculates that reinforcement depends on activation of a "go mechanism" which promotes learning by supplying a burst of extra excitation (arousal?). This new view has a great deal in common with the ways in which several other contemporary writers have begun to think, as well as with hypotheses suggested by some pioneers of learning theory quite a while ago and with current Russian conceptions of the physiological processes behind conditioning. Reinforcing agents commonly involve a rise in excitation or arousal, quickly followed by a fall. The rapid succession of the rise and the fall may account for our failure so far to disentangle the roles of these two phases.

What is going to happen in motivation theory over the next ten years? What solutions will impending psychological and physiological experimentation give to the still unsolved problem of the nature of reinforcement? Will the concepts of "drive" and "arousal" be swept aside, will they fragment into several distinct and more precisely defined concepts, or will they be firmly vindicated and more accurately pinpointed? Will motivation theory lose such autonomy as it now possesses or will its boundaries become clearer? The only prediction that it would be safe to venture is that anybody who continues to read the Nebraska Symposium for the next ten years will know the answers.

AMERICAN SCIENTIST, *52*, 1964

HUMINOIDS ON OTHER PLANETS?

by ROBERT BIERI

"THE PROBABILITY of evolving some living system was likely high. That evolution would go in a particular direction is a different matter. Thus the *a priori* probability of evolving man must have been extremely small—for there were an almost infinite number of other possibilities. Even the probability of an organism evolving with a nervous system like ours, was, I think, extremely small because of the enormous number of alternatives. I am therefore not at all hopeful that we will ever establish communication with living things on other planets, even though there may well be many such on many planets" [1].

I think that at least a few biologists would disagree with all but the first sentence of this statement. Because of continued interest in possible interplanetary communication, I would like to present arguments for the opposite view, namely, that if life has evolved on other planets in other solar systems and if some population has reached the level of conceptual thought, it is highly probable that the organisms so endowed will bear a strong resemblance to *Homo sapiens*.

Essentially this argument is based on the premise that the physical properties of the elements, the forms of energy available, and the environmental conditions which would allow life to arise and evolve are such that severe limitations are imposed on the number of routes available to evolving forms. The number of alternative possibilities is by no means infinite; on the contrary, the number is quite limited. This limited number of available routes has led to the innumerable cases of convergent evolution in plants and animals.

Man is the result of a series of evolutionary "breakthroughs"; he is the result of the solution of a number of major evolutionary problems. Only a few of these are discussed here in order to illustrate the validity of the premise of limited solutions or routes available at each turn in the long evolution from abiogenetically produced organic matter to conceptualizing being.

In this discussion, life is defined as a system of matter having heritability and mutability. Such a system of matter requires periodic or continual capture of matter and energy. As long ago as 1913, Henderson [2] advanced the argument that, of all the elements known, only carbon fulfills the many requirements necessary for the existence of a duplicating, living system. He further argues that such a system could only develop in water. Urey [3] has recently reaffirmed this idea. This argument is now so generally accepted by scientists that it is seldom emphasized that it is a major limit on the number of evolutionary pathways available to life [4]. More recently, biochemists have argued that a living system based on carbon must also be based on the high energy phosphate bond. The fact that the ATP-ADP system is common to all

living systems illustrates the limited range of biochemical alternatives [5]. The construction of living systems on a carbon framework in water means that we are limited to a temperature range not widely different from that of our earth.

Because of the limited time available, on the order of five to ten billion years, and because of other limiting factors, it is most probable that a living system will arise within a liquid medium after chemical contributions have been made from the solid-liquid and liquid-gas interfaces. The emergence of life from a solid or gaseous system, although perhaps not impossible, certainly is highly improbable.

If a living system is to evolve to any significant degree of complexity, some form of autotrophic organism must evolve to support the primary heterotrophic forms. Because of the greater energy capture possible, photosynthesis and an oxidizing atmosphere will eventually supersede the many possible chemosynthetic autotrophic energy systems.

In the long evolution within a sea that must precede invasion of the land, herbivores and carnivores will have reached a high degree of complexity and specialization. Both types of organisms will be bilaterally symmetrical because the viscosity of the medium demands streamlining for speed of pursuit, speed of escape, and the efficient use of captured energy. There are many radially symmetrical animals in the earth's oceans. However, nearly all of these have evolved from bilaterally symmetrical ancestors after adopting a sessile mode of life. The Radiolaria among the protozoa and the majority of the coelenterates represent animals that probably show primary radial symmetry. They are good examples of the low level of organization attained by radially symmetrical animals. Probably the best examples of secondarily derived radial symmetry are found in the annelids. Many modifications accompany sessile living but the most important for the present discussion are the reduction of sensory structures and degeneration of the nervous system. Only an active mode of life, the pursuit of prey and escape from predators, has led to larger, more complex nervous systems [6].

Active search for food and pursuit of prey have resulted in a bilaterally symmetrical animal with an anterior mouth and a posterior anus [7]. Invariably the mouth is surrounded by sensory and grasping organs. There may be small sense organs scattered along the body and at the trailing end, but in essentially all cases known in the animal kingdom the leading surface of the bilateral animal has the largest and greatest variety of sensing devices [8]. This is not because of some accident in the dim past, only one of many possibilities. The anterior mouth with surrounding sense and grasping organs has been independently evolved in group after group, again and again. It is not surprising therefore to find the largest ganglion or the brain at the front end in close proximity to the major sensing organs.

An organism with brain and sense organs at the front end has several major adaptive advantages. First, it takes time to send sensed data over nerves and the successful animal is not one that sends the data to its tail and then back to the grasping or feeding organs. Secondly, the ganglion or brain is the integrating center that evaluates various incoming signals and sends out the command signal. To save space, to reduce the chance of damage, and to reduce interference and noise, it is more efficient to have short sense conductors and long command conductors. Thus, the brain and major sense organs are close together. Even in highly efficient two-directional animals such as the squids, the major sense organs are grouped at one end of the body next to the brain and near the mouth. The major sense organs do not migrate to distant parts of the body nor does the brain.

Thus, we can be more than reasonably sure that advanced animals will be bilaterally symmetrical, and will have a large brain at the front end near the mouth in close proximity to the largest and most diverse sense organs.

Given that life will first arise in a liquid medium, water, we can be sure there will be a long evolution in the liquid environment before any population of organisms is able to invade and occupy the solid-gas interface. It is conceivable that an aquatic population might leave its ocean milieu by way of a subterranean route or directly penetrate the air from the ocean surface. However, if we recall that an invading population needs an energy source, it becomes apparent that both of these possibilities are very remote. Also, we can probably rule out the possibility of flying plants or burrowing chemosynthetic autotrophs. Permanent invasion and extensive utilization of the land-air interface require terrestrial plants, autotrophs, decomposing heterotrophs, and terrestrial animals, heterotrophs, evolving together in interdependent communities. The air, in all probability, can only be occupied after the land-air interface and by way of the land-air interface. Although some fish and squids are able to make short journeys into the air, all true fliers have evolved from the land. The aquatic insects have secondarily invaded the water from the land.

On the earth, the evolution of the largest brain occurred at the solid-air interface. We now know that at least five different chemical synaptic transmitters are used in various nerve tissues. It seems highly probable that the higher forms of terrestrial life have adopted the most efficient of these and have achieved the greatest miniaturization of nerve cells and conductive tissue possible. If this supposition is correct, large integrating centers, brains, of a size similar to ours will be necessary for conceptual thought [9]. If this assumption is accepted, we can be reasonably certain that a large brain will not develop underground or in the aerial habitat.

Recent work on the dolphins [10] indicates that we cannot be as certain that the largest brain on other earths would not develop in an aquatic medium. However, I think that this is highly unlikely for the following reasons. It is now reasonably well established that man's large brain evolved concurrently with social behavior, the use of tools, and speech [11]. If any one of these had been suppressed, *Homo* would never have reached the level of development of conceptual thought. Although social behavior and speech or at least communication would have and have had great adaptive and survival value in the sea, the development of tools in a liquid medium is highly improbable because of the density and viscosity of water. The sea otter rises to the surface to break his fragile, sea urchin food on his chest while floating on his back. It is very difficult to slam one's fist against one's chest while completely submerged in water. Using a lever to pry rocks or food from the bottom while completely submerged is also difficult. The simplest tools, thrown rocks and sticks, would be useless in the sea. Thus, on the basis that tools would be much less effective under water than on land it is highly probable that the largest-brained animal will evolve at the air-land interface [12]. Similarly the use of tools while flying is a very remote possibility.

This then raises the question of locomotion on land. There are many answers and solutions to this problem including sliding on slime, wriggling, and walking with legs. There can be no doubt that the latter method is superior to all others when we consider the friction involved, the energy required, and the speed and maneuverability obtainable. It is quite significant that wheels, one of man's greatest achievements, have never been evolved in any living organism. The primary reason for this is that living tissue just is not suitable for the high pressures that must be sustained at the bearing of a wheel. On land there is the further limiting factor of an inadequate amount of flat surface on which to use wheels. Thus, again, the number of possible structures is quite limited. Legs will be required.

We come then to the question of how many legs, one, two, three, four, six, ten, one thousand? The fossil record shows that there are strong selective pressures in both the marine and terrestrial environments that lead to the reduction of multiple appendages. This is so well demonstrated that it is given the distinction of a "law," Williston's Law. We can probably eliminate the odd number of appendages approached in many different animal groups but never really achieved. There are the three "pointed" kangaroos, the seven "legged" Collembola or spring tails, the five "armed" platyrrhine monkeys and many other examples. All of these can, for various reasons, be eliminated as real challengers to the paired appendage body plan. Although there are all numbers of paired appendaged organisms living on the earth, all but the vertebrates

with two pairs and the insects with three pairs are essentially back water fugitives of only moderate or less success (with apologies to the acarologists and their eight-legged friends). Just why the higher vertebrates ended up with four legs is another tale that takes us back to the liquid medium of their early evolution. As higher and higher speed swimmers evolved, stabilizing and steering appendages of various sorts were developed [13]. The details of why four came out on top is only briefly discussed in vertebrate zoology treatises although this surely was a fundamental necessity for the vertebrate invasion of the land [14].

Just why the insects have six legs is not immediately apparent but is perhaps related to their small size and exoskeleton of chitin. At any rate, it seems most probable that our extraterrestrial huminoid will have either two or three sets of paired appendages. I'm willing to bet on the smaller number.

Multiple sense organs with diverse functions have often been postulated for extraterrestrial animals. Almost certainly, outer space huminoids will have sensors for light (vision), sensors for chemicals in solution or dispersed in air (taste and smell), and for pressure changes (hearing). Whether they will have sensors for magnetic fields is less certain [15]. Strong arguments can be advanced for the presence of only two eyes set for binocular vision and two ears for binaural hearing. We can also argue strongly for the smell sensor being directly over the mouth. Its primary function in animals with high visual acuity, and probably in most others, is to test the nature of materials about to enter the mouth, is it poisonous or edible?

Could the conceptualizing organism have additional sense organs based on other parts of the electromagnetic spectrum besides the visual portion? Wald [16], on the basis of biochemical limitations and energy requirements, argues no. Certainly, the ability to detect infrared at night would have great survival value. An acoustical ranging system similar to that of bats might be present, in which case the ears would be proportionately larger than ours. However, in this case the eyes might be reduced somewhat. On the other hand, any marked reduction in the visual sensors would be a serious impediment to the evolution of tools and the associated evolution of a large brain.

What about the dactyls? Although the human hand is considered to be a relatively unspecialized vertebrate structure, it is hard to visualize a more effective arrangement. Three, four, six, or seven fingers might work as well, but certainly an arrangement of claws, fingers, tweezers, knives, and so forth would not work as well. If we accept two as the minimum number and ten as the maximum, then four, five, or six dactyls would seem most probable and our extraterrestrial huminoid will count by eights, tens, or twelves.

We might consider the nature of the outer covering. Will it have scales, feathers, hair, foam, a cellophane wrapper? I would suspect that a competent vertebrate zoologist consulting with a biochemist could make a strong argument for skin and hair.

No doubt many other possibilities have occurred to the reader and the task of answering them is endless. I have considered and rejected green skin, reproductive organs on the chest, antenna systems, a ventral nerve cord, different temperature regimens, stronger and weaker gravitational systems and many other possibilities.

Those who wish to suggest alternatives not considered here, should realize that, if their suggested modifications of the extraterrestrial huminoids are to be considered seriously, a reasonable set of evolutionary steps must be set up to explain the final structures derived.

To restate the argument, a conceptualizing population of living organisms can only develop by the process of evolution. Given the ninety-two known, naturally occurring elements, the forms of energy available, and limited time, the number of alternative solutions to the major steps leading to a conceptual organism are strictly limited. The phenomenon of convergent evolution is so widespread in both the plant and animal kingdoms that it needs no special elucidation here. Suffice it to say that the evidence shows that, again and again, animals and plants have independently evolved not only similar structures but also similar biochemical systems and similar behavioral patterns as solutions to the same fundamental problems.

If we fail to communicate with conceptualizing beings on other planets it will not be because they are fundamentally different from us but because they have either far surpassed our state of technology and have no interest in communicating with us or have not yet reached our state of advancement and are thus unable to do so. This probably is the fundamental factor which would greatly reduce the number of such populations attempting to communicate with us.

If we ever succeed in communicating with conceptualizing beings in outer space, they won't be spheres, pyramids, cubes, or pancakes. In all probability they will look an awful lot like us.

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VALENCY AND THE CHEMICAL BOND

By J. W. LINNETT

In 1916, G. N. Lewis proposed that a group of *two* electrons and a group of *eight* electrons in atoms and molecules showed a particularly high stability. These groupings have since come to be known as the *electron-pair* and the *octet of electrons*. For example, the hydrogen atom containing one electron was very reactive, but the helium atom containing two was very unreactive, illustrating the stability of the pair. The atoms containing 3 (lithium), 4 (beryllium), 5 (boron), 6 (carbon), 7 (nitrogen), 8 (oxygen) and 9 (fluorine) were all more reactive than helium but that containing 10 electrons (neon) was unreactive, like helium. This, together with other experimental facts, led to the view that the electrons in an atom were present in shells, the neon atom containing an inner shell of two (the helium pair) together with an outer shell of eight (an outer octet). Similarly, an argon atom with eighteen electrons is unreactive and is presumed to contain the two neon shells consisting of a pair and an octet together with a third shell which also contains an octet.

Further, Lewis proposed that, in forming ionic compounds such as sodium fluoride consisting of Na^+ and F^-, the sodium atom (11 electrons) had lost an electron to give a sodium ion, with the electronic structure of neon and also containing a pair and an octet. A fluorine atom had gained the electron to give a fluoride ion also containing a pair and an octet (10 electrons in all, like neon). Many other salts (ionic compounds) could be explained in a similar way; for example, NaCl, KF, MgF_2, MgO, Li_2O, CaO and BeF_2.

However, there are, of course, other compounds such as methane, water and chlorine monoxide which are not ionic. In these compounds the atoms are bound together in molecules, the pattern of linking and spatial arrangement of the component atoms within the molecule being well defined. In these cases, Lewis proposed that the component atoms attained the electronic structures of the inert gas atoms, consisting of pairs and octets, by *sharing* electrons between one another. For example, the formulae of methane (CH_4) and water (H_2O) were written as

$$\begin{array}{c} \mathrm{H} \\ \mathrm{H}:\ddot{\mathrm{C}}:\mathrm{H} \\ \ddot{\mathrm{H}} \end{array} \qquad \begin{array}{c} \\ :\ddot{\mathrm{O}}:\mathrm{H} \\ \ddot{\mathrm{H}} \end{array}$$

The inner electron-pair of the carbon and oxygen atoms (i.e., the helium pair) were omitted from these diagrams and only the outer shell (also called the valence-shell) was included. Thus, in the methane molecule, the carbon atom attained the octet of the neon atom by acquiring a *share* of the four electrons of the four hydrogen atoms. These, together with

the four it possessed already, completed the octet. But, likewise, each hydrogen atom acquired a share of one of the four electrons provided by the carbon atom. As a result of the sharing of the four electron-pairs each hydrogen atom had associated with it two electrons (like helium) and the carbon atom had an octet shell made up of four pairs. In the water molecule, each hydrogen had a shared pair and the oxygen an octet consisting of two shared pairs and two lone pairs (six of the eight electrons were contributed by the oxygen atom and two by the two hydrogen atoms). Formulae of many other molecules may be written in a similar way. For example:

methyl alcohol	ethylene	fluorine	fluorine monoxide	hydrogen

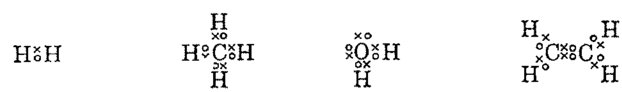
The only additional feature in this list is that, in the ethylene molecule, the two carbon atoms share two pairs.

Spatially, the pairs in the octet were supposed to be in regions disposed at the corners of a regular tetrahedron which had the nucleus at the center. It was realized that there might be some distortion of this tetrahedron if the pairs bound different atoms, or if some of the pairs of the octet were shared and some were lone. Thus, the hydrogen atoms in the methane molecule are at the corners of a regular tetrahedron. In the water molecule, the four pairs may be supposed to be at the corners of a slightly distorted tetrahedron; the HOH angle in the water molecule is 104.5°, whereas the HCH angle in the methane molecule is 109.5°. In F_2O, the FOF angle is 103°, while in Cl_2O the ClOCl angle is about 111° presumably because of the greater mutual repulsion of the large chlorine atoms. None of these differ greatly from the regular tetrahedral angle (109.5°).[1] The G. N. Lewis octet has come therefore to be visualized as four pairs tetrahedrally disposed round a central nucleus.

Electron Spin: About ten years after G. N. Lewis's 1916 paper, it was shown that the electron had an intrinsic magnetic moment which has been ascribed to it possessing a *spin*. Experiment showed that, in a magnetic field, the magnetic moment could only align itself in two ways with respect to the field. Thus, if there were two electrons in an atomic system, there were in general two possibilities; either the magnetic moments of the two electrons could be aligned relative to one another so that the resultant magnetic field was zero, in which case the spins were said to be opposed; or the two magnets were aligned in such a way that the field

[1] The smallish variations in these angles have been discussed by C. E. MELLISH and J. W. LINNETT, *Trans. Faraday Soc.*, *50*, 665 (1954) and by R. J. GILLESPIE and R. S. NYHOLM, *Quart. Rev. Soc.*, *11*, 339 (1957).

was greater than for one alone, in which case the spins were said to be parallel. Because, in helium, the resultant magnetic moment due to spin is observed to be zero, the spins of this stable pair must be opposed. Likewise, the resultant electron spin magnetic moment of the hydrogen molecule is zero, so that, in the pair forming the two-electron bond in the molecule of hydrogen, the spins are opposed. Further, the resultant electron spin magnetic moments of the neon atom and of the molecules of methane, ammonia, and water are all zero. This suggests that the octet is made of four electrons of one spin and four of the other, the electrons occurring in pairs, one of each spin. Thus if we use a cross (×) to represent an electron of one spin and a circle (○) to represent an electron of the opposite spin, the molecules H_2, CH_4, C_2H_4 and H_2O would be represented by



Electrons in Atoms and Molecules: Electrons are negatively charged. Consequently, they repel one another and tend, because of this, in the lowest energy state, to keep apart. However, they are also attracted by the positive nuclei. As a result, they will stay near the nuclei but, because of their mutual repulsion, will space themselves round the nuclei.

Electrons in atomic and molecular systems possess kinetic energy; they cannot be regarded as stationary. However, for fundamental reasons, it is impossible to follow the actual motions of these electrons. Consequently, it is not possible to assign electrons to particular positions in an atom or molecule but only to regions. (This is the Heisenberg Uncertainty Principle.) Let us call these regions *domains*. When the location and form of a domain occupied by an electron produces an enhancement of the probability of that electron being situated between the nuclei, then that negatively charged electron serves to bind the two positive nuclei together, and it is a bonding electron.[2] What has been described earlier as a *shared*-electron is of this type, whereas an electron in a lone-pair is not.

There is another effect which governs the relative positions in space that two electrons will occupy. It may be expressed in simple terms, as follows: electrons which have the same spin have a low probability of being near one another. That is, electrons having the same spin, have a tendency to keep apart, this being an effect additional to their electrostatic repulsion. This effect of spin on the spatial distribution of electrons in atoms and molecules is just as important as the effect of their electrostatic repulsion.

[2] An alternative way of stating this is to say that, because the negatively charged electron in the molecule is under the influence of *two* positive nuclei, its energy is lower than in an atom where it was under the influence of only one nucleus.

Let us consider a set of electrons of the same spin in an atom or molecule. As has been said, it will not be possible to assign them to exact positions but only to domains. Further, all the electrons of this set will tend to keep apart because of the combined effects of charge and spin. The resulting arrangement can be reasonably described in terms of a set of domains, each occupied by one electron of the set, these domains being to a high degree mutually exclusive in a spatial sense. That is, a satisfactory description of the distribution of the electrons of this spin-set can be obtained by dividing the space in the atom or molecule into a set of separate non-overlapping domains. The assumption of strictly non-overlapping domains is over-rigid and is an approximation, but a useful one. Moreover, for qualitative considerations, this is not at all a serious fault. The reason for this is presumably that the qualitative effects of charge and spin are the same for the electrons of this spin set.

Let us consider the five electrons of one spin in the methane molecule. Consider the region round the carbon atom. One electron will occupy a domain close to, and including, the nucleus; this domain is best regarded as spherical. This is the electron which is a member of the pair forming the inner shell. Round this, for the next shell (the four of the octet) the space may be divided into four equivalent segments. In order that these domains may be as little restricted as possible in *any* direction, and so that the four electrons may be as widely separated as possible they will be disposed tetrahedrally round the nucleus and the innermost domain. The four electrons occupying these four domains are thus arranged so that the protons bound by them will tend to be tetrahedrally disposed round the carbon nucleus.[3]

But, in all chemically important polyelectronic systems there are a number of electrons having one spin and others having spins opposed to those of the first set. Considering first the electrons of the second spin-set alone, these will keep apart from one another by the effects of charge and spin spatial-correlation in a manner similar to the electrons of the first set. That is, to a reasonable approximation, the space can be divided again into a set of five exclusive domains each occupied by one electron.

The relation between the electrons of the two spin-sets must now be considered. Here the effects of charge and spin differ. This contrasts with effects of charge and spin within the same spin-set, for, in that case, the charge and spin correlation effect operate in the same sense (i.e., both operate to keep the electrons apart). Between the electrons of the two spin-sets, charge, of course, still operates by way of electrostatic repulsion to keep the electrons apart. As regards the correlating effects of spin, and

[3] Those who have used the directed orbital method of valency will appreciate the relation between these domains and the tetrahedral sp^3 hybrid orbitals. In fact, these four tetrahedral hybrids are a way of describing the domains though, in that orbitals case, the regions where the orbitals tail-off would overlap.

considering ground states only (this article will be limited to the consideration of ground states), if there are an equal number of electrons of each spin (i.e., two equivalent spin-sets), then the spin correlation effects operate against the effect of electrostatic repulsion in such a way as to produce a tendency for the electrons to be present as spatial-pairs. That is, the effect of spin-correlation is such as to favour the domains for the electrons of one spin-set being coincident with the domains for the electrons of the other spin-set. However, if the numbers of electrons in the two spin-sets are different (e.g., in the boron atom or nitric oxide molecule) then there is no spin-correlation effect between the electrons of the two spin-sets, and each spin-set will adopt an independent set of domains, their relative disposition being decided, for a given arrangement of nuclei, by the effects of interelectron repulsion.

Let us return to a consideration of the methane molecule. We saw that, for the first spin-set, the five electrons could be regarded as occupying one inner spherical domain and four equivalent outer tetrahedrally-disposed domains. The five electrons of the second spin-set will occupy a similar set of five domains. Now, interelectronic electrostatic repulsion will operate to cause one tetrahedral set to be "staggered" with respect to the other set. However, spin-correlation will tend to cause the two sets of domains to be identically disposed. But the four protons (the presence of which makes the total charge on the species zero) are directed by each spin-set to be tetrahedrally disposed round the carbon (see earlier). Consequently, they operate so as to cause the domains for one spin-set to be identical with those of the other spin-set. The precise consequences on the electron distribution of these conflicting effects is, of course, impossible to assess and, as a consequence, no exact treatment of the methane molecule has as yet been achieved. However, as an approximation, one may say that two effects operate to cause the domains of the two spin-sets to be identical (spin correlation and the effect of the protons) while only one (interelectron repulsion) separates them. Therefore, a very satisfactory approximate "picture" is to regard the domains as identical, each occupied by two electrons, one of each spin, there being some correlation within each domain of the position of the two electrons. By this means, the protons are kept as widely separated as possible, and yet close to the electrons that "bind" them, while the electrons are also kept apart as widely as possible.

Thus, the reason why the methane molecule is tetrahedral is that charge and spin-correlation operate to keep the electrons of a given spin-set as far apart as is reasonable, considering that they are simultaneously attracted by the nuclei of the system. As a result, the two spin-sets of four electrons which make up the octet each adopt a tetrahedral arrangement round the carbon. Therefore the four protons, which are bound by those electrons, will be tetrahedrally disposed, and the presence of the

protons tends to cause the two tetrahedral spin-sets, which make up the octet, to be similarly disposed in space. Hence, in this molecule it is possible to say that each proton is held by a pair of electrons, one of each spin; that is, by an electron pair bond. So, for methane, the octet may be regarded *either* as four tetrahedrally disposed electron pairs, as was done by G. N. Lewis, *or* as two similarly disposed tetrahedral spin-sets each of four electrons. For methane there is no difference between the two descriptions.

The electronic structures of the ammonia and water molecules may be described in a very analogous manner. Again, there are five electrons in each spin-set. The two spin-sets are similar to the two in methane but, for example in H_2O, two of the pairs serve to bind two protons, while two of the pairs are lone pairs. The four outer tetrahedrally disposed domains are no longer equivalent. Two of the domains contain in addition to two electrons, a proton, while the other two contain just two electrons. The electrostatic attraction of the protons on the pair of *bonding* electrons causes a contraction of those domains (because such a contraction results in a lowering of the potential energy) relative to the domains containing the lone pairs, which can be enlarged and so reduce both the kinetic energy and also the electrostatic repulsion between the electrons occupying a lone-pair domain. As a result of this angular contraction of the bonding-pair domains relative to the lone-pair domains, the HOH angle in water (104.5°) is less than the regular tetrahedral angle (109.5°).

In the case of a molecule like CH_3OH it is easy to visualize in a general way the form of the nine domains, each containing an electron pair. Both the carbon and oxygen will have an inner domain round the nucleus and each will have four domains round them. One domain will be common to the outer set of the carbon atom and the outer set of the oxygen atom, and this will contain the pair of electrons, one of each spin, which bind the carbon and oxygen nuclei together. The other three outer domains of the carbon atom will each contain an electron pair and a proton, as will one of the outer domains of the oxygen atom. The other two outer domains of the oxygen atom will both contain lone pairs. The diagrammatic formula for CH_3OH given earlier provides a good representation of this arrangement, though it must, of course, be remembered as well that the four outer domains and electron pairs round the carbon atom, and also those round the oxygen atom, are both tetrahedrally disposed a sa result of the effects of charge and spin correlation and the energetic advantage in binding the nuclei together as effectively as possible. Again the octet round the carbon atom may be described either as four pairs tetrahedrally disposed or as two spin-sets of four each tetrahedrally disposed. The same is true for the oxygen atom.

In the case of the ethylene molecule, there are eight domains including the two inner ones containing the carbon nuclei. Each carbon atom has

four domains round it. Two domains are common to the two carbon atoms, being separated from one another by a plane which is the plane of the six nuclei. Thus the "picture" is of each carbon having an outer octet made up of four electron pairs, two pairs being used to bind two hydrogens, and two being shared with the other carbon atoms. The disposition of the pairs is still tetrahedral but, because *two* pairs are used to bind the one carbon atom to the other, in the double bond, the regular tetrahedral arrangement is distorted.

These two pairs will be drawn towards one another, and hence the extent of the domains they occupy will be reduced. Therefore, the other domains become enlarged and the HCH angle becomes greater than the tetrahedral (it is 117°). Again the octets round each carbon may be treated as two quartets. Each spin-set of four having a tetrahedral disposition and, because of the need to lower the potential energy as much as possible, and bind the protons as strongly as possible, the two spin-sets (two quartets) are similarly disposed.

Nitric Oxide and Oxygen: In the nitric oxide molecule there are 15 electrons and experiment shows that there is one more having one spin than the other. We may suppose that, for both the nitrogen and oxygen atoms there are inner pairs occupying domains round the nuclei. Of the remaining 11 electrons, six will have one spin and five the other. Crosses (×) and circles (○) will be used to represent them as before. Then the six electrons can provide four having this spin-tetrahedrally round each nucleus by an arrangement in which two are shared at the apices of two tetrahedra with a common edge (the "centres" of the domains being at the apices of two tetrahedra with a common edge):

$$\substack{\times\\\times}\mathrm{N}\substack{\times\\\times}\mathrm{O}\substack{\times\\\times}$$

The five electrons of the other spin can provide four having this spin tetrahedrally round each nucleus by an arrangement in which three are shared (the "centres" of the domains being at the apices of two tetrahedra with a common face):

$$\circ\mathrm{N}\substack{\circ\\\circ\\\circ}\mathrm{O}\;\circ$$

The total formula is then:

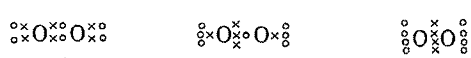
$$\circ\substack{\times\\\times}\mathrm{N}\substack{\times\\\times}\substack{\circ\\\circ\\\circ}\mathrm{O}\substack{\times\\\times}\circ$$

Each atom has round it an octet of electrons, four of each spin. However, the situation is different from those we have considered so far in that the arrangement of electrons of one spin is different from that of the other.

Let us now consider the domains for the eleven valence-shell electrons. For the set of six electrons (crosses) the spatial arrangement of the

domains will be like that in ethylene, but each domain will contain only one electron. For the five electrons, the domains will be quite different though, of course, overlapping the first set and, in fact, extending, in all, over the same spatial region. Of the five domains, three will be common to the two nuclei, the fourth will be associated with the nitrogen atom and the fifth with the oxygen atom. Round each nucleus the "centres" of the domains of each set will be disposed at the corners of a tetrahedron slightly distorted from a regular one. Thus, there is one set of six domains for the electrons of one spin and another set of five for the electrons of the other spin. Because the "centres" of the domains are differently disposed, the electrons of one spin are kept apart from the electrons of the other spin as well as from the others of the same set. Consequently, the interelectronic repulsion energy is reduced, and this reduction contributes to the stability of the molecule. It is probably a factor in rendering the monomer (NO) more stable than the dimer O=N—N=O.[4] The strength and length of the NO bond are consistent with the view that binding is provided by 5 electrons.

In O_2 there are sixteen electrons, four of which, as two pairs, will occupy domains round the two nuclei. For the remaining twelve there are a number of possibilities: (a) six electrons of each spin; (b) seven of one and five of the other; (c) eight of one and four of the other. Chemical formulae which would apparently associate four electrons of each spin with both nuclei are:



(a) (b) (c)

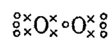
Let us take (c) first. We have said earlier that, for four electrons of a given spin the satisfactory disposition of the domains is that the "centres" should be located at the apices of a tetrahedron having the nucleus at the centre. But it is impossible for this to be so and also for the four to be shared *between* the two nuclei. Therefore (c) cannot in fact provide four *bonding* electrons in the way that it will be seen that (a) and (b) can.[5] Formula (c) is therefore unsatisfactory compared with (a) and (b) because the number of bonding electrons is smaller. As regards (a) we have seen that a set of six electrons can be arranged so that there is a tetrahedral pattern round each nucleus, the two tetrahedra having a common edge between the nuclei. Both sets of six could therefore have this arrangement so that (a) is a formula which satisfactorily provides four bonding electrons. For (b), the five electrons can be arranged at the

[4] Such a dimer is unknown though other associations of two NO molecules are known.

[5] For the same reason, we can have single, double and triple bonds, but not quadruple bonds.

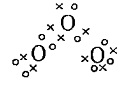
apices of two tetrahedra which have a common face situated between the nuclei (*cf.* one set in NO). The seven electrons can be arranged at the apices of two tetrahedra which have a common apex between the nuclei (*cf.* the two sets in F_2). Both (a) and (b) therefore provide satisfactorily four bonding electrons, but (b) will be preferred energetically because the electrons of one spin (and the domains they occupy) are arranged in space differently from those of the other spin (and the domains they occupy). This separation of the electrons of one spin from those of the other, means that the interelectron repulsion energy will be reduced, so that the energy of (b) will be lower than that of (a). Consequently, molecular oxygen will be paramagnetic, because the magnetism arising from the electrons of one spin does not cancel that arising from the electrons of the other spin. Molecular oxygen is unusual in this respect. The length and strength of the bond in O_2 is consistent with a bond consisting of four electrons. Formula (a) corresponds with a low-lying excited state.

In solid potassium superoxide KO_2, the ion O_2^- exists as a separate entity. Omitting the inner shells of electrons the structure of this will be expected to be

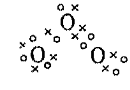


Each atom has an octet of electrons made up of four electrons of one spin and four of the other. The internuclear distance (1.28 Å) is a little greater than that in O_2 (1.21 Å), the difference being consistent with a change from a bond consisting of four electrons in O_2 to one of three in O_2^-. It is interesting that this ion is stable in crystals in which the positive ions are large and carry only a single positive charge (K^+, Rb^+ or Cs^+) and the field is weak. When the electron field is stronger with the smaller positive ions Li^+, and Na^+, and the more highly charged ones Ca^{++}, Sr^{++} and Ba^{++}, the more highly charged $O_2^=$ and $O^=$ are stabilized.

Ozone, Carbon Dioxide and Nitrogen Dioxide: In the molecule of ozone there are twenty-four electrons, six of which are as three pairs in domains near the three nuclei. The possible Lewis structures for the remaining eighteen electrons are:



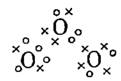
I



II

In each structure every atom has an octet made up of four pairs of electrons and therefore four electrons of each spin. Because ozone is symmetrical (and for other reasons), Pauling suggested that ozone should be represented as a "resonance hybrid" or combination of the two above structures. But, on the revised octet (double-quartet) hypothesis a third

structure is possible:

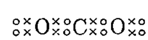


III

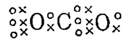
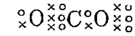
Calculations by R. D. Gould and M. H. Booth suggest strongly that this last formula (III) provides a better representation of the structure of ozone than a hybrid of I and II. The reason for this is not far to seek. By locating the domains for the electrons of one spin differently from those for the electrons of the other spin, the interelectronic repulsion energy for III is lower than for I and II—and the real molecule in the ground, or lowest, energy state will clearly achieve such a reduction. Also, in I the electrons are concentrated at one end of the molecule, whereas in II they are concentrated at the other. Hence, with resonance, an attempt is being made to describe the electronic structure of ozone in terms of two structures both of which have this same unsatisfactory feature. (Excess electron concentration at an end oxygen atom.) In III the electron distribution is more even, and the concentration of electronic charge to one end or the other is not as great as in I or II.

Consequently, in the present hypothesis, the stability of ozone relative to what would be expected for I is not ascribed to the existence of "resonance between I and II" but to the reduction in interelectron repulsion existing for structure III compared with that for either I or II. It should be noted that the OO bond lengths in O_2^- and O_3 are 1.29 Å. On the present hypothesis both bonds would be described as involving three electrons. The nitrite ion will be expected to have a similar structure to that of ozone.

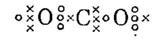
The carbon dioxide molecule has two fewer electrons than ozone, so that there are 16 in the atomic outer shells. The formula that is usually ascribed to CO_2 is



though Pauling, to account for the stability, suggested a resonance hybrid of this with

 and 

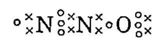
the latter two making smaller contributions. On the double-quartet hypothesis an electron-arrangement which will be expected to reduce interelectronic repulsion compared with these will be



Each atom has a double-quartet of electrons, and there are the same number of electrons of each spin, so that the formula is in satisfactory

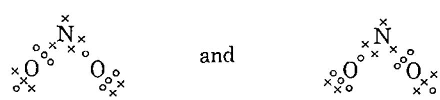
agreement with the diamagnetism of CO_2. Again the stabilization of CO_2, relative to what is to be expected for the formula O=C=O, is ascribed to the achieving of a reduction in interelectronic repulsion without any loss in the number of bonding electrons.

For iso-electronic N_2O, an analogous formula would give to the terminal nitrogen atom an excessive number of electrons and a more probable structure is

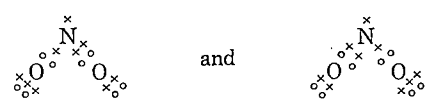


The lengths of the NN and NO bonds (1.13 and 1.19 Å) are consistent with this structure (*cf.* N_2^+: 1.12 Å; CH_3NO_2: 1.22 ± 0.02 Å). The above structure disposes the domains for the electrons of one spin differently from the domains for the electrons of the other spin, so reducing interelectronic repulsion.

The molecule NO_2 contains one more electron than CO_2 and one less than O_3. This suggests that the electronic structure would be a hybrid of



with



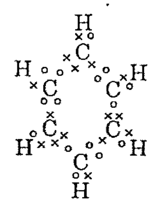
perhaps being also of some importance. The upper two are likely to be more important because it would appear that, for them, interelectron repulsion energy would be less than for the disposition of electrons represented by the lower two formulae.

Now there is an interesting feature about the above electronic structures which is that, in all cases, the electrons of one spin (the crosses) favour a bent molecule while the electrons of the other spin (circles) favour a linear molecule. This is apparent if the tetrahedral dispositions of the four domains for the electrons of one spin for each atom are examined. Now all the well-known triatomic molecules or ions except NO_2 are either linear or have interbond angles between 90 and 120°. But NO_2 has an ONO angle of 134°. This unique value is easily understood on the basis that the angle is between the 180° favoured by one spin-set and the 90–120° favoured by the other spin-set.

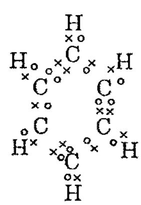
Benzene: The hypothesis being presented here proposes that the electrons should be considered as two spin-sets, each assigning to the neighbourhood of every carbon, nitrogen, oxygen, and fluorine atoms, four electrons, occupying separate domains, which are disposed tetrahedrally

round the nucleus. The most important structures will be those which, within these limitations, (*a*) have as many bonding electrons as possible; (*b*) separate the electrons of the two spin sets as much as possible; (*c*) do not build up large positive or negative charges in any region (*cf.* the previous discussion of O_3, and also N_2O).

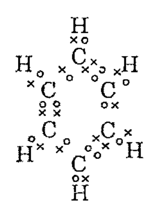
On this basis the structure that will be expected for benzene is:



Each carbon atom has a double-quartet of electrons and the geometry allows the four domains of each set at every carbon atom to be tetrahedrally disposed without distortion. The domains of one set are disposed differently from those of the other, so interelectron repulsion is reduced. The distribution of the electrons is very uniform and suitable to the nuclear charges involved. Calculations of a limited kind that have been carried out by P. B. Empedocles for this formula (electron distribution) indicate that it does give a better representation of the electronic structure of benzene than does the more conventional "resonance hybrid" of two Kekulé structures



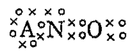
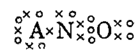
and



These imply a greater interelectronic repulsion energy than does the structure involving six "three-electron bonds" and hence are likely to provide a less good representation of the lowest energy state.

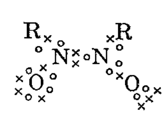
Nitroso-Compounds: The alkyl and aryl nitroso compounds (e.g., HNO, CH_3NO, and C_6H_5NO) are different from the halogen compounds: FNO, ClNO, and BrNO. For instance, the former dimerise while the latter do not. Physical measurements show that the NO bond in the first group of compounds is longer and weaker than that in FNO, etc., which more resembles that in NO itself. Also the FN, ClN, and BrN bonds in the halogen compounds are very much *longer* (0.15 to 0.2 Å) than equivalent bonds in other molecules which clearly consist of an electron pair (e.g., in NF_3 or NH_2Cl). Two electronic structures seem reasonable possibilities (having regard to distribution of electronic charge) for the nitroso com-

pounds ANO:

 and 

The former achieves a very even charge distribution, and there is no concentration of electrons in excess of the local nuclear charge anywhere. However, because the AN bonding involves a pair, which must be localized in the same domain, the domains in the NO part of the molecule will be the same for both the spin-sets; thus, while the charge distribution is very satisfactory, repulsion between the electrons of the two spin-sets is considerable. On the other hand, in the second structure, the two sets of domains are disposed differently so that interelectron repulsion is less. On the other hand, there is a greater concentration of electrons on A, and a smaller one on the oxygen atom. In HNO, CH_3NO and C_6H_5NO, the groups, H, CH_3 and C_6H_5 cannot readily assume a negative charge; that is, too great an energy is involved in their doing so. Consequently, these molecules adopt the first structure despite the greater interelectron repulsion. But F, Cl, and Br, which all have a considerable electron affinity as atoms, can assume a negative charge easily (i.e., with little increase in energy) so they adopt the second structure and profit, as a result, from the reduction in electronic repulsion. This explains why, in the former compounds (HNO etc.), the characteristic NO vibration frequency is 1550 cm^{-1} while in the halogen compounds it is about 1800 cm^{-1}; why the NO bond length in HNO is 1.21 Å but in the three halogen compounds is 1.13, 1.14, and 1.15 (*cf.* 1.15 in NO); why, as stated earlier, the nitrogen-halogen bonds are all unexpectedly long; why CH_3NO dimerises whereas FNO, etc., do not.

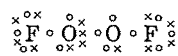
The last item is particularly interesting. A satisfactory formula for the dimer of RNO which separates the two spin sets is possible. This will mean that there is a reduction in interelectron repulsion without the assignment of any excessive concentration of electrons to the alkyl or aryl groups, or any reduction in the number of bonding electrons. This formula for the dimer is



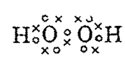
It must of course be combined with the mirror image structure. The observed NN bond length is consistent with this formula. Thus, the driving force for dimerization is a reduction in interelectron repulsion energy because the structure for the monomer, which disposes electrons in pairs in the domains (i.e., conventional Lewis formula), is replaced by a structure which separates the domains of the two sets, and hence also the electrons. For FNO, etc., no such advantage can be gained by dimeriza-

tion because, already in the monomer, the two sets of domains are different. This means that it is being proposed that the alkyl and aryl nitroso compounds dimerize *because* the monomer have "conventional" Lewis structures (and electron distributions) while FNO, etc., do not dimerize, but remain in the monomoric form, because the monomer does not have a "conventional" Lewis structure, but one which separates the electrons of one spin-set from those of the other.

A similar effect presumably accounts for the unexpectedly short OO bond in FOOF (1.22 Å. *cf.* O_2: 1.21 and HOOH 1.48) and the unexpectedly long FO bond (1.58 Å *cf.* F_2O 1.41). The structure

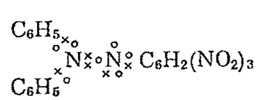


reduces electronic-repulsion by separating the domains of one spin-set from those of the other. This is achieved by creating a somewhat high concentration of electrons on each fluorine atom. This is allowable for a fluorine atom, but a hydrogen atom cannot assume such a large share of the electrons, so H_2O_2 has the structure



and the bonds are "normal" electron-pair bonds.

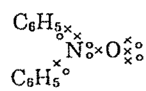
Free Radicals: There are some free radicals (i.e., species containing an odd number of electrons) which are particularly stable; and unexpectedly so on the basis of the classical Lewis electronic theory of valency because each atom cannot have as its valence shell an octet made up of four electron pairs. One of these stable free radicals is diphenyl-picryl-hydrazyl which can be kept for long periods and does not dimerize. On the present hypothesis, the electronic arrangement in the vital NN region can be represented by

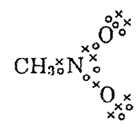


Each nitrogen has an octet (double-quartet) of electrons. Moreover, if the consequences of dimerization are examined, it will be found that there would be *no* increase in the number of bonding electrons but there would be an increase in interelectron repulsion because the two sets of domains would become identical. The above formula is consistent with the electron spin resonance spectrum which shows that the odd electron is equally divided between the two nitrogen atoms.

Another free radical which shows a similar stability is diphenyl nitric oxide. Crystals of the dimethoxy derivative have been studied by x-ray diffraction and the NO bond length (1.23 Å) is virtually the same as in

nitromethane (1.22 Å). The electronic formulae of these "molecules" would be represented by

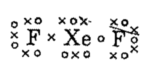




so that the equality of lengths is understandable. The NO characteristic frequency (1350 cm^{-1}) of $(C_6H_5)_2NO$ is between that in $CH_3—N{=}O$ (1550) and that in $(CH_3)_3N{\rightarrow}O$ (950) which is consistent with the bond being a three-electron one.

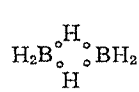
The stability of other free radicals such as semiquinones, Würsters blue cation and viologens, can be accounted for in a similar manner. The stability of radicals like triphenyl-methyl however cannot be accounted for in this way. It seems to the writer that the stability of this type of radical is a consequence of there being *many* resonance structures. A consideration of the reason why this should confer stability is beyond the scope of the present article.

Xenon Difluoride and Diborane: Calculations by Miss Bilham suggest that, the most satisfactory formula for this molecule is

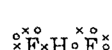


each atom having an octet. In this formulation the xenon carries an excess positive charge, and the fluorines carry excess negative charges. The molecule is linear because one tetrahedral spin-set on the xenon is staggered with respect to the other; one electron in each spin-set is bonding and because of fluorine-fluorine non-bonded repulsion, electrons which are directly opposite one another are employed. The two spin-sets round both fluorines are also staggered with respect to one another. Consequently, interelectronic repulsion energy is low in this molecule.

Calculations by B. J. Duke indicate that the most satisfactory chemical formula for the ring in diborane is



there being four one-electron bonds. The structures of other boron hydrides can also be formulated using one-electron bonds. Another similar example is the formula of the hydrogen bond in the HF_2^- ion which can be written as



again a structure involving two one-electron bonds is used.

Conclusion

The object of the present article has been to try to bring out those features which, it seems to the writer, are basic for considering the qualitative behaviour of electrons in atoms and molecules. These are (*a*) the electrostatic attraction between nuclei and electrons and the potential energy associated with this; (*b*) the fact that electrons cannot be localized but can only be associated with regions of space (here called domains) and the additional fact that the extent of a region is associated with the kinetic energy of the electron occupying that region, the kinetic energy being smaller the larger the region; (*c*) the consequences of the Pauli Principle which, with the additional factor of interelectron repulsion, makes it possible to treat the domains for electrons of the same spin, to a satisfactory approximation, as being virtually exclusive; (*d*) the effect of interelectron repulsion between electrons having opposite spins which tends to keep the two spin-sets apart and to mean that, if the two spin-sets are separated, the potential energy is reduced.

In this article the domains have not been described in terms of hybridized atomic orbitals (though this is probably the most satisfactory way of describing them). This was done because it seems to the author that the use of hybridized orbitals can easily obscure, to some degree, the more fundamental features of the behaviour of electrons which are summarized for us by the Pauli Principle (i.e., the detail and technique can obscure the principle).

This article has been limited to molecules in which the atoms (except hydrogen) acquire a share of eight electrons; the octet. For atoms of higher atomic number it seems that the valence shell can contain more than eight electrons. The ideas presented here can be extended to such systems by considering for instance, in addition to double-quartets, double-pentets, double-sextets, etc., of electrons.

The way of regarding the electronic systems which is presented in this article is basically of the type usually described as valence-bond in that attention is focused on the local situation in the neighbourhood of each atom. However, in the conventional valence-bond method, the electrons are considered in pairs; lone pairs and shared pairs. That is, only those structures are considered for molecules containing an even number of electrons which assign two, one of each spin, to each domain. These are the conventional structures proposed by G. N. Lewis, and improved descriptions have been proposed by Pauling and others which make use of resonance between (i.e., combinations of) these Lewis-structures. However, it is proposed here that structures in which the electrons of one spin occupy a different set of domains from those of the other are allowable. This means that the octet is to be considered as being made up of two spin-sets of four electrons, each tetrahedrally disposed round the

nucleus. The two spin-sets may be disposed in coincident domains, but, on the other hand, it is possible, in many systems, for the domains of one spin-set to be disposed differently from those of the other spin-set. Moreover, it is proposed that, because thereby interelectron repulsion is reduced, structures in which the two sets of domains are not coincident can be of particular importance.

AMERICAN SCIENTIST, 52, 1964

DISPERSION STRENGTHENING OF METALS

By RICHARD B. ELLIS

Designers of nuclear power plants, hypersonic aircraft, and space vehicles are continually seeking materials that have high strength at elevated temperatures. These requirements have been met in part by the precipitation-strengthened "superalloys" that are suitable for applications up to around 1800°F. The refractory metals, tungsten, molybdenum, columbium, and tantalum, may sometimes be used when the service temperature exceeds the useful temperature of the superalloys. However, the refractory metals are expensive, difficult to fabricate, and have poor resistance to oxidation.

There is a limit to which one can extend the service temperature by precipitation strengthening. In precipitation-strengthened alloys, a phase that is soluble in the matrix at temperatures well below the melting point comes out of solid solution as a fine-grained dispersion during a heat treatment. Consequently, the strength of the alloy is lowered as the temperature rises into the range where the dispersed phase goes back into solution.

Within recent years, considerable effort has been devoted to the development of dispersion-strengthened alloys in which the dispersed phase is insoluble in the matrix. There are some striking similarities as well as some important distinctions between precipitation and dispersion strengthening alloys. In both materials, the dispersed phase consists of very small particles (<0.001 inch in diameter) distributed uniformly throughout a metal matrix. A significant difference is that precipitation-strengthened alloys are melted, cast, and heat-treated in conventional procedures, whereas dispersion-strengthened alloys are generally prepared by powder-metallurgical techniques.

The dispersion-strengthening process is so new that there are only two types of dispersion-strengthened metals on the market commercially. Aluminum alloys made by powder metallurgy are being used in the nuclear power field as structural members and for sheathing fuel rods in some reactors, and in components for jet engines. TD Nickel, the other product, is used in a number of aerospace applications, including jet engines, where its superior temperature resistance will permit operation at higher temperatures with a resulting increase in efficiency. Other potential applications appear in the area of high-temperature heat exchangers and regeneratively cooled rocket engines.

In general, dispersion-strengthened alloys will be chosen where service temperatures are higher because they will have usable strength up to 80

to 90 per cent of the melting point of the base alloy instead of the 65 to 70 per cent limit of the precipitation-strengthened alloys. This boost means extending the use of nickel from about 1800° to about 2400°F., or the use of aluminum from 500 to 900°F.

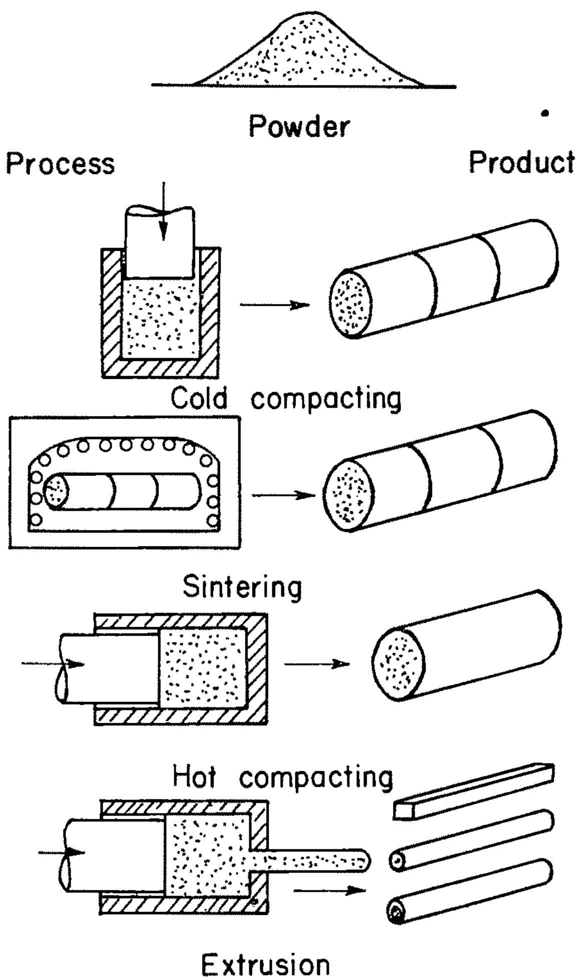


FIG. 1. Steps in manufacture of SAP products (after Bloch[9]).

History

Research which eventually led to the dispersion-strengthening process began as early as 1909 in Europe. Experiments with various aluminum powders in 1922 led Franz Sauerwald [1] to the conclusion that the oxide film that forms on aluminum surfaces interfered with pressing and sinter-

ing of the powders to such an extent that a coherent body could not be obtained. This concept was accepted for almost 20 years until the metallurgist Max Stern [2] described the effect of the oxide film in strengthening the end product. In a 1940 patent, he disclosed a process which may be considered basic to the production of dispersed alloys in two respects. He used particulate aluminum and magnesium scrap—turnings, filings, grinding dust, etc.—and he used hot pressing, hot forging, and hot extrusion at temperatures up to 900°F. to rupture the oxide film on the particles and to obtain metal-to-metal contact.

The dispersion-strengthening phenomenon was first observed in products made from sintered aluminum powder (SAP) by the Swiss metallurgists Alfred von Zeerleder [3] and Roland Irmann [4]. Their development was announced in 1949 by Aluminium Industrie Aktien-Gesellschaft. In connection with the discovery, von Zeerleder [3] said in 1955, "When making standard specimen rods for spectrographic analysis by mixing pure aluminum and other elements in powder form, Dr. Irmann and his associate P. Nater were impressed by the high hardness of the hot-pressed rods. Detailed mechanical tests revealed the surprising fact that even specimen rods prepared from unalloyed powder showed mechanical strength values which approached those of alloys." It was soon realized that the high strength of these specimens was due to the particles of oxide from the surface of the powder that became distributed throughout the body by the compacting and extrusion processes used in making the rods. Extensive research followed in the laboratories of the AIAG in Switzerland and of the Aluminum Company of America in this country, which uses the name Aluminum Powder Metallurgy (APM) Products.

It was immediately recognized that the potentialities of this discovery were not limited to aluminum products. Numerous investigations into a variety of systems have been made, especially by Nicholas Grant of Massachusetts Institute of Technology, Fritz Lenel of Rensselaer Polytechnic Institute, Claus Goetzel of New York University, and their respective co-workers.

Methods of Preparation

The general procedure for the production of SAP and APM products is illustrated in Figure 1. Various modifications of this process have been used. For instance, the sintering and hot-compacting steps, or the hot-compacting and extrusion steps, may be combined, or the extrusion may be replaced with other forms of hot-working such as forging, swaging, or rolling. The starting powder is generally a flake type, prepared by rolling and stamping until the metal becomes brittle from work-hardening and breaks up into flakes. The flakes are ball-milled to achieve the desired particle size and oxide content. The milling is a combination of comminution and welding. The comminution exposes fresh fracture surfaces

which promptly become coated with an oxide film approximately 100 Å thick. Later, these surfaces become welded together under the impact of the balls and the oxide films are broken into fragments that become embedded in the metal matrix. The oxide that forms during preparation of the powder is a hydrous, amorphous form. During sintering, the oxide loses water and crystallizes, and the evolved water reacts with metal to form more oxide. These reactions are important in establishing a strong bond between the metal matrix and the dispersed particles, which contributes to the strength of the final product. Thus, it is a fortunate happenstance that the properties of the oxide coating are just right to give a dispersion with properties that are just right for strengthening. Attempts to apply this principle of surface oxidation to other metals such as magnesium and titanium have had only limited success.

A variation of the SAP process is one in which an intermetallic compound rather than the metal oxide is used to strengthen the alloy. In this process the powder is made by atomization of a molten aluminum alloy containing iron, nickel, or manganese. The very rapid chilling of the atomized droplets produces a very finely dispersed precipitate of an intermetallic compound, $NiAl_3$, $FeAl_3$, or $MnAl_6$. This powder is comparatively low in oxide, 0.5 to 1.0 per cent. When this powder is processed as before, the resulting alloy is strengthened by the dispersed precipitate of the intermetallic compound.

Another method that has been developed well enough to go into commercial production is based on co-precipitation of mixed oxides, followed by selective reduction of the less stable oxide. The precipitation step may involve insoluble compounds that decompose to oxides on heating, e.g., hydroxides, carbonates, or oxalates. The commercial product is Du Pont's TD-2 Nickel, a 2 per cent by volume dispersion of thoria (thorium dioxide) in nickel. Nickel hydroxide is precipitated from a solution containing a soluble nickel salt and a colloidal suspension of thoria. The precipitate is dried, granulated, and fired in hydrogen to give a powder of nickel metal, each particle of which contains dispersed thoria. This powder is processed by conventional techniques into rod or sheet without affecting the degree of dispersion of the thoria.

An obvious variation of the surface-oxidation technique is to apply to a metal powder a coating of some compound other than its own oxide. For instance, the oxides that form on copper, nickel, and stainless steel are not stable enough to serve as the dispersoid, but the metals are desirable as the matrix. Coatings of stable oxides, carbides, nitrides, borides, etc., have been applied to powdered metals, but the resulting products have not been as satisfactory as the SAP-APM type. One drawback is that, in order to obtain a suitably fine dispersion, the metal particles should be of sub-micron size and each particle must be uniformly coated. It is very difficult to keep such small particles from agglomerat-

ing, which results in nonuniform coating. Another problem is that in many cases the bonding between matrix and dispersoid is unsatisfactory.

The method most often used in exploring new combinations is to mix powders of the desired metal and disperse phase (nickel and alumina, for example), followed by the usual powder metallurgical practice of compaction, sintering, and working. This method is versatile in the number of combinations that may be tried. For example, the oxide of aluminum,

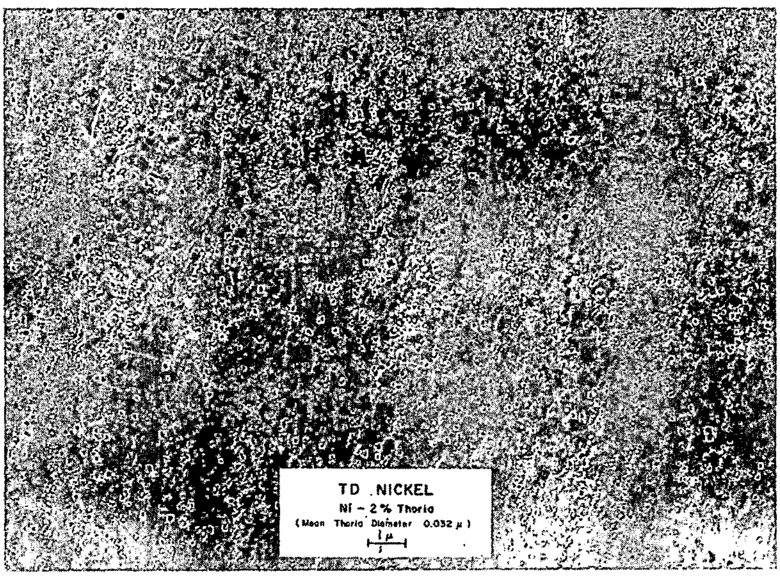


FIG. 2. Electron micrograph of TD-Nickel mag. 5000×. Courtesy E. I. duPont de Nemours & Co.

magnesium, thorium, zirconium, or titanium might be used as the disperse phase, and iron, nickel, cobalt, copper, aluminum, magnesium, lead, gold, or stainless steel as the base metal. There are problems in obtaining uniform dispersions because of the great tendency of very fine powders to agglomerate, and uniformity is very important to strength.

Excellent results have been produced in certain cases by a method that is called internal oxidation because it involves diffusion of oxygen into a solid solution of a reactive metal in a more noble metal matrix, such as aluminum in copper. The chosen alloy is powdered and then heated in an atmosphere of controlled oxygen content. After the oxidation step is complete, the powder is heated briefly in hydrogen to reduce surface oxides which would interfere with the subsequent compaction and sintering procedures. The reactive metal is preferentially oxidized, resulting in a very fine dispersion of the oxide in the noble metal, e.g., aluminum

oxide in copper. These products show the same degree of enhancement in strength and hardness as do the SAP products. However, the diffusion process is slow so that the preparation is tedious and expensive. A similar but less successful technique is to zone-melt a nickel-aluminum alloy in an atmosphere containing some oxygen, giving a dispersion of aluminum oxide in nickel. The time that the matrix is molten must be kept to a minimum to avoid agglomeration of the alumina.

If a copper-thorium alloy is mixed molten with a copper-boron alloy, thorium boride precipitates as a fine dispersion. This process is similar to the precipitation-strengthening processes, but differs in that the ThB precipitate will not redissolve on heating. This idea has also been applied by mixing two powders of the same matrix metal but containing members of a reacting pair. Solid-state diffusion results in precipitation of an intermetallic compound. Obviously, the process is slower than when molten alloys are used. Busk and Leontis [5] mixed powders of Mg-0.6%Zr and Mg-36%Al. After compacting, sintering, and hot-working, a heat-treatment caused precipitation of $AlZr_3$. These approaches to obtaining stable dispersions appear to hold promise for more interesting developments.

The newest technique for obtaining strengthening dispersions involves electrodeposition. Sub-micron-sized alumina particles are dispersed in a Watts-type nickel-plating bath and become embedded in the deposited nickel without agglomeration. Zirconia and thoria have likewise been dispersed in electrodeposited nickel. Other variations are alumina in nickel-cobalt alloys, in cobalt-tungsten alloys, and in gold. At the present stage of development there are limits on the sizes of samples attained, but there are no inherent restrictions to prevent eventual production of many useful articles. There are obvious advantages to being able to electroform directly to desired shapes rather than starting with billets.

Strengthening Mechanisms

The increased strength of dispersion-strengthened alloys results from the interference of the dispersed particles with the movement of dislocations through the crystal lattice.

According to current theory, deformation of a metal structure takes place by the movement of dislocations, which are line defects in the crystal lattice resulting from a difference in the number of rows of atoms on the two sides of a slip plane, as illustrated in Figure 3. In Figure 4, the dislocation has jumped one cell unit to the right by a simple flip in atomic bonding, stimulated by a shearing force. Figure 5 shows the unit of slip produced when the dislocation reaches the edge of the crystal. Many such slips will lead to a visible deformation.

Barriers to dislocation movement range in size from single atoms up to micron-sized inclusions. The strengthening effect of solute elements has

been known since primitive times, but a suitable explanation of the mechanism had to await the development of dislocation theory. The presence of a foreign atom is enough of a discontinuity in the lattice to retard the movement of a dislocation past it.

The presence of discrete particles of a separate phase, such as occurs in dispersion strengthening, acts on a larger scale in much the same manner

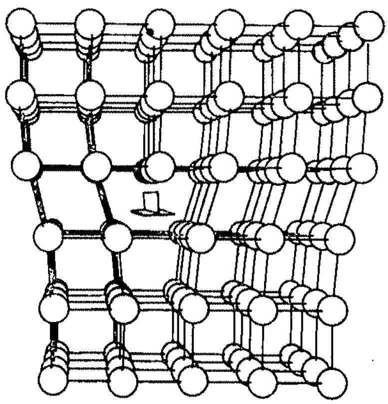


FIG. 3. Dislocation in a cubic crystal (after Zackay[10]).

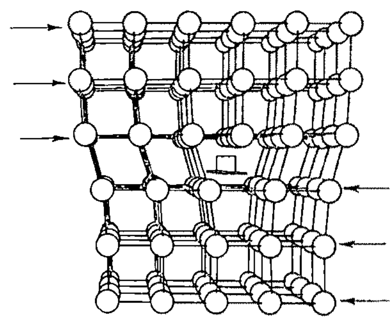


FIG. 4. Dislocation has moved one atomic unit to the right, under influence of a small shearing force indicated by arrows (after Zackay[10]).

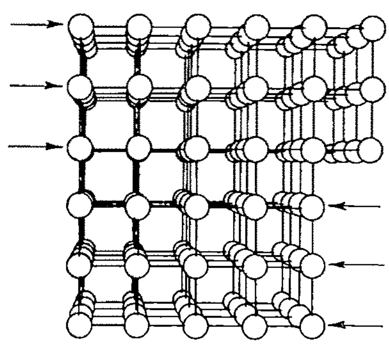


FIG. 5. Dislocation has reached edge of crystal, producing a unit of slip (after Zackay[10]).

to impede the motion of dislocation lines. There are variations in detail of the mechanism depending on the nature, size, and distribution of the dispersed particles.

There have been a number of theories proposed to explain the mechanism of dispersion strengthening, each with some virtues and some defi-

ciencies. The theory generally accepted at present was proposed by Lenel and Ansell [6] of Rensselaer Polytechnic Institute in 1960. They suggest that the first dislocation passes between the particles, leaving a dislocation loop around each. Successive dislocations pile up around the particles until the accumulated stress causes them to yield or fracture. The yield stress of the alloy is inversely proportional to the square root of the interparticle spacing and is directly proportional to the square root of the yield strength or the shear modulus of the particle, depending on whether the particle deforms or fractures. In the case of a very fine dispersion, the dislocations in the pile-up are not straight, but curved and there will be an enhanced strengthening effect due to an added contribution from the curved dislocation line energy. To get very small interparticle spacing without an excessive volume fraction of dispersoid, the particle size must also be very small.

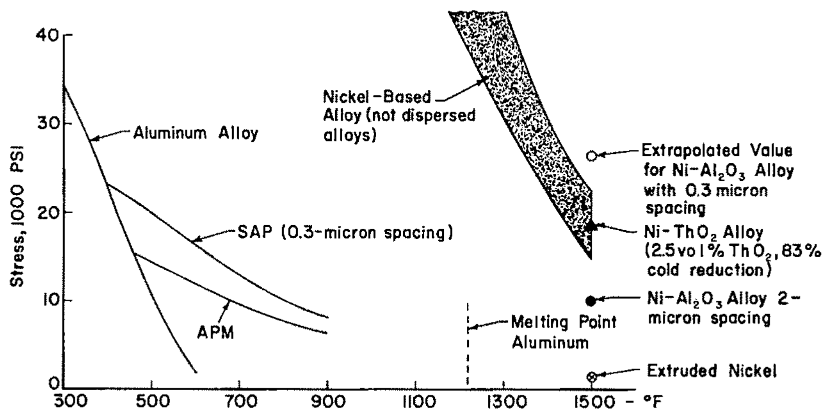


FIG. 6. Stress—temperature curves for rupture in 1000 hours; data from ref. 11 and 12.

In order to attain high strength at high temperatures, the matrix metal should have a high melting point and a high strength to begin with. If it is to impart additional strength, the material of the dispersed phase should have a high thermal stability in contact with the matrix, a low solubility in the matrix, a low diffusivity in the matrix, and a high strength. The dispersion should consist of a uniform distribution of particles less than one micron in size, preferably 100 to 1000 Å, with an interparticle spacing of less than one micron. There is little known about the effects of particle shape; some researchers favor a plate-like shape. Lenel and Ansell's theory specifies that there must be wetting or bonding between the matrix and the dispersoid. There are examples of some strengthening effects in systems in which there appears to be little or no bonding, but these effects are proportionally much lower than the effects in bonding systems.

Strength Improvement

The improved behavior at elevated temperatures of SAP alloys is shown in Figure 6 by comparison with ordinary aluminum alloys. The behavior of SAP has grown to be accepted as the first major plateau in grading efforts to improve other systems. Since aluminum has a low melting point, the behavior of SAP can only be compared to high-temperature systems by some method that rationalizes the difference in temperatures. This has been done in Figure 7 where the 0.25% offset yield strength of various systems can be compared to that of SAP. The yield strengths of the different systems are compared at homologous temperatures, i.e., levels that are equal fractions of the systems' melting points.

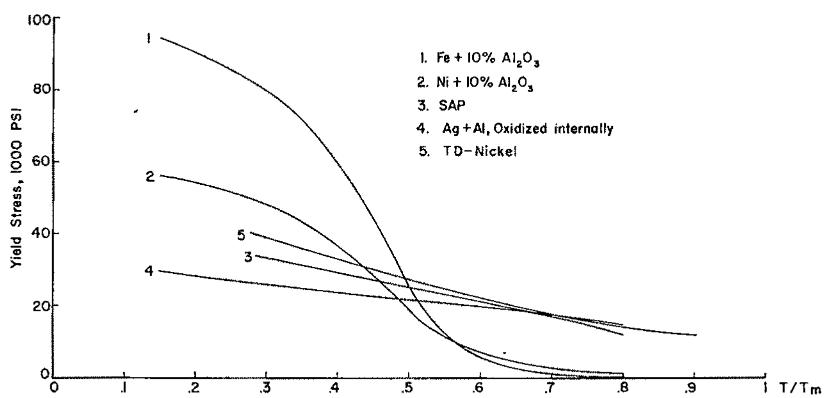


FIG. 7. 0.25% offset yield strength—homologous temperature T = test temperature °K, T_m = melting point °K; data from ref. 13 and 14.

The importance of interparticle distance in developing strength has been shown by measuring the stress required to rupture in 1000 hours at 1500°F. for a series of nickel-aluminum oxide alloys with varying interparticle distance. The values ranged from 3000 psi at 10-micron separation to 10,000 psi at 2-micron separation.* The distance between particles of aluminum oxide in SAP is approximately 0.3 micron. If the work that has been done with nickel-dispersoid systems were extrapolated to give the same interparticle distance as that of aluminum oxide particles in SAP, then the strength of the nickel would be improved to a degree comparable to that of aluminum. This point is illustrated in Figure 6.

* In comparison, ordinary nickel has a strength of 2000 psi under the same conditions.

The Future

There are several unsolved problems to interest researchers. A tantalizing one is the observation by Sherby [7] that SAP alloys show no discontinuity in creep strength at the melting point of aluminum. How can a dispersion fail to go fluid when the matrix melts? Sherby could only suggest that the aluminum oxide particles must form an interconnecting network. Peiffer [8] has observed the same phenomenon in dispersions of alumina in silver. However, in both cases, electron microscope pictures indicate that the particles are not interconnected. No explanation has yet been discovered.

At the present state of development, dispersion-strengthened alloys must be made by powder metallurgy techniques. They cannot be melted for casting because the dispersed phase agglomerates very quickly when

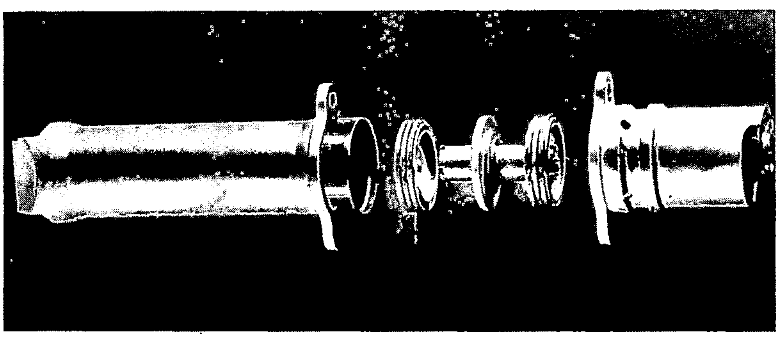


FIG. 8. Afterburner activating cylinder assembly for the J75 jet engine. These APM parts are used at temperatures as high as 900°F. Courtesy Aluminum Company of America.

the matrix is molten. If this disadvantage could be overcome, it would be possible to make the alloys by incorporating the disperse phase as a powder into a melt of the matrix metal. This method would be much easier and cheaper than the present methods involving lengthy pretreatment of powders followed by the several steps of compaction, sintering, and hot-working.

The present production methods limit the choice of matrix to a single metal or a simple alloy. Hence, the strengthening effects of a disperse phase have not yet been combined with other strengthening mechanisms. Some dispersion-strengthened alloys can be work-hardened to some extent, but the increase in strength is not very important. The superposition of dispersion-strengthening on the strength of present superalloys is an area of considerable current interest. Here again the possibility of forming the dispersion in the molten state would be a great advantage.

A serious limitation in the use of dispersion-strengthened alloys is the

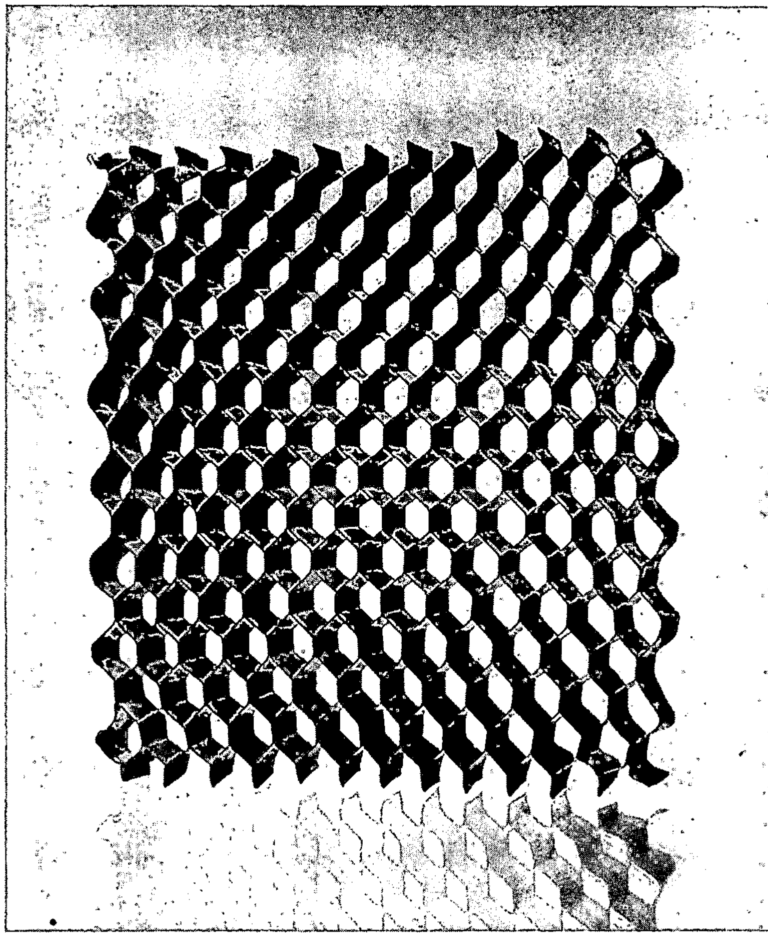


FIG. 9. One of the interesting new applications of Du Pont's TD nickel sheet is this honeycomb structure manufactured for an aerospace application. Layers of 2-mil thick sheet were joined by diffusion bonding to achieve the end result. (Fabrication by Hexcel Corp., Berkeley, Calif.).

fact that a welded joint loses the enhanced strength of the alloy as a result of agglomeration of the dispersoid in the part that was molten.

Continuing work in the field of dispersion strengthening will result in improved products and a better understanding of the strengthening mechanism. The products will be used in more efficient heat engines, faster supersonic aircraft, and cheaper manufacturing processes where high temperature is a controlling factor.

Acknowledgments

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America in granting permission to reproduce figures is gratefully acknowledged.

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AMERICAN SCIENTIST, *52*, 1964

POLAR RISE AND EQUATORIAL FALL OF SEA LEVEL*

By A. J. EARDLEY

STUDY of submerged and emerged shorelines, and of the depths of continental shelves, suggests that sea level has risen in the Arctic and fallen in the equatorial regions. The amount seems to be in the range of 600 ft. This is a measurable effect since the Cretaceous.

The cause is thought to be a slowing-down of the earth's rotation, due to the drag of the lunar tides. The equatorial bulge of the solid mantle and crust has lagged in adjustment to the slower rate of rotation, but the ocean waters have adjusted immmediately.

North America

East Coast: The east coast of North America has long been recognized as one along which general emergence has occurred southward of Long Island and submergence northward. A coastal plain of Upper Cretaceous and Cenozoic sediments appears on Long Island and broadens southward to Georgia and the Gulf Coast, where its inner margin is at least 600 ft above present sea level, and before erosion may have been higher. From Long Island northward the Paleozoic orogenic belts become progressively embayed by the ocean waters northward through the Gulf of Maine, the Gulf of St. Lawrence, and the Grand Banks of Newfoundland. The Cretaceous and Tertiary sediments are completely overlapped by the advancing sea, and lie submerged on the broad continental shelf. The Arctic margin of North America, 700 miles broad, is half submerged (see Figure 1).

A graph of the depth of the outer margin of the Atlantic continental shelf of North America, shown in Figure 2, indicates that the break in slope becomes progressively deeper from low latitudes to high. At Lat. 30°N it is approximately 600 ft deep, and at Lat. 82°N in the Arctic it is 1300–2000 ft deep. The Mississippi delta is abnormal and should not be considered in this connection, nor should Florida, which has had a local history of subsidence and downtilting to the south throughout Cretaceous and Cenozoic time.

The depths of incision in the continental shelf of the Hudson submarine valley, the St. Lawrence valley, and the broad valleys in the shelf between Spitzbergen and Novaya Zemlya are also shown on Figure 2, and are progressively deeper from south to north. They suggest either a progressive subsidence of the land or a rise in sea level from Lat. 30°N

* Presidential address, National Association of Geology Teachers, presented at New York, November 20, 1963.

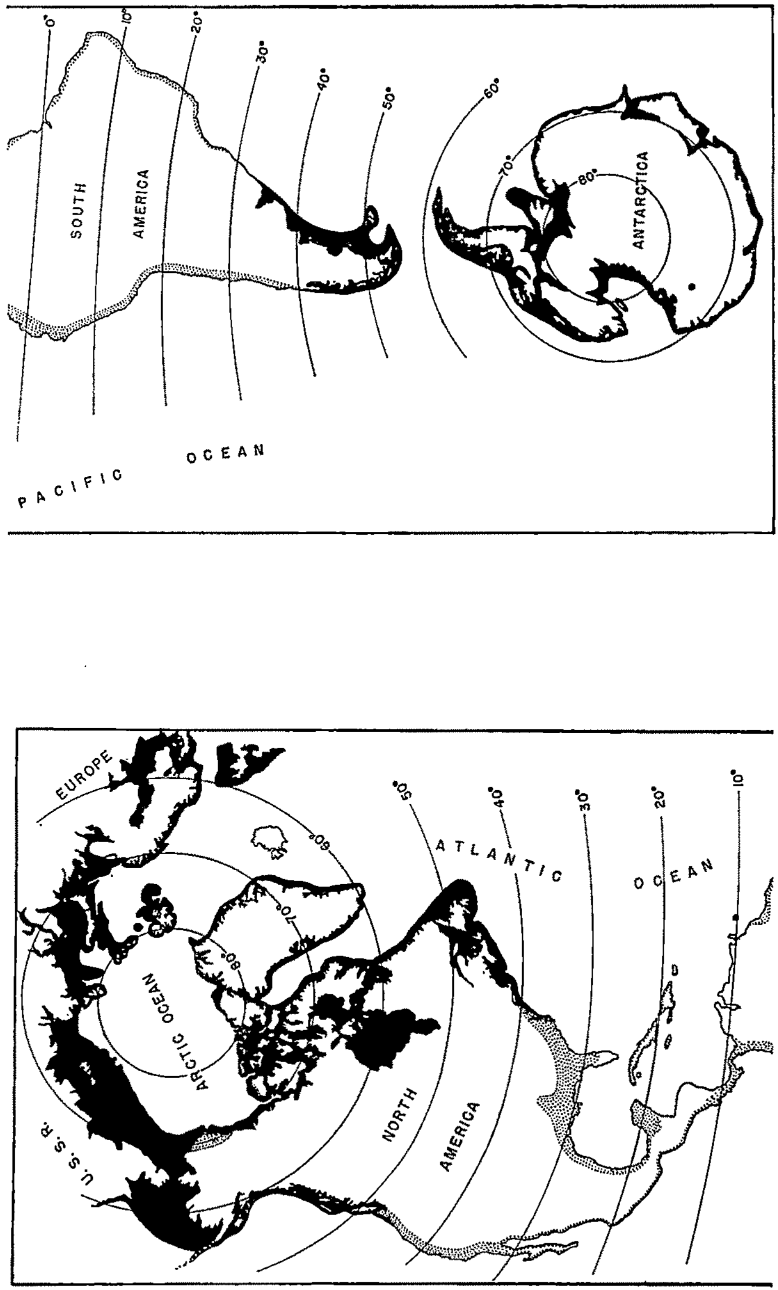

FIG. 1. Map of the western hemisphere, with the Arctic region and the continent of Antarctica added, showing in black the approximate extent of encroachment of the sea over the continental borders during the Cenozoic and by stippling the shorelines of emergence.

to Lat. 82°N. The entrenched Hudson valley in the shelf does not extend clearly to the shelf margin and hence its depth at the outer margin is rather indefinite.

West Coast: Puget Sound marks the change on the Pacific Coast from fairly bold, straight shorelines on the south to embayment on the north (Figure 1). The fiord land of British Columbia and southeastern Alaska

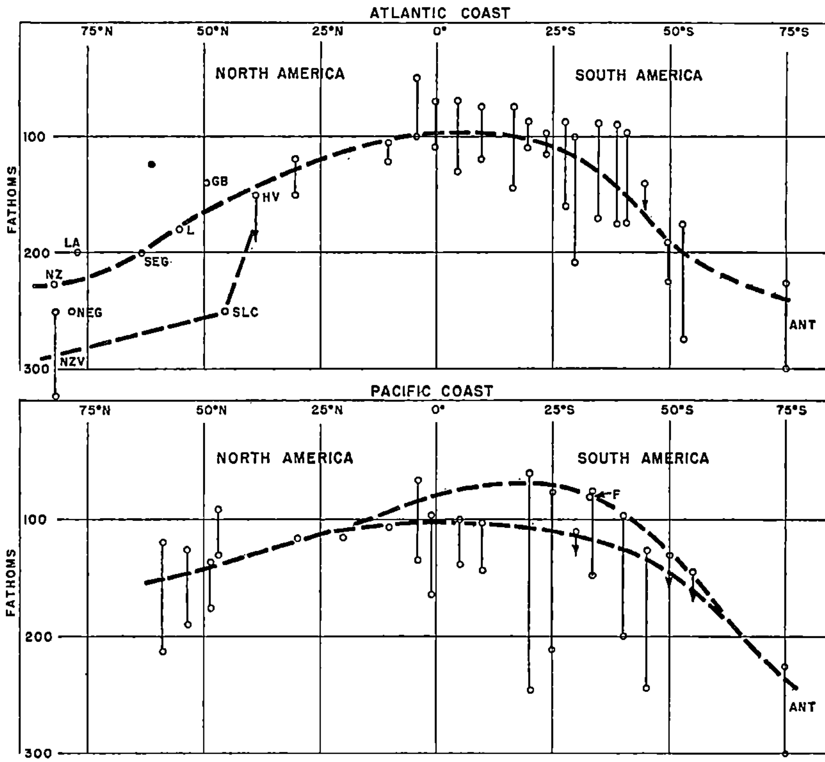

FIG. 2. Depth of outer margin of continental shelf. Top chart is along Atlantic coast of North and South America and bottom chart is along western coast. Data chiefly from U.S. Navy Hydrographic charts. The single circles indicate a fairly definite fix; the connected bars indicate the closest range determinable within which the break in slope occurs; the arrowed circles indicate the upper limit but not the lower. GB, Grand Banks; HV, Hudson submerged valley; L, Labrador; SEG, southeast Greenland; NEG, northeast Greenland; LA, Laptev Sea; NZ, Novaya Zemlya; SLC, St. Lawrence submerged valley; NZV, Novaya Zemlya submerged valley; ANT, Antarctica; F, Datum from echo-sounding profile off Valparaiso.

is well known for deep sea-flooded valleys that dissect the continental shelf and margin for more than 100 miles inland. The shelf margin, and the depth of incision of the numerous valleys in the shelf, do not show a progressive deepening to the north, at least between Puget Sound (48°N) and Cross Sound (58°N); this may be due to the extensive glacial erosion and sedimentation in the region (Figure 2). The crust along this shore has adjusted upward 600 ft since Wisconsin time as a result of glacial melting, but even with complete melting of the glaciers and total isostatic adjustment, the coast would remain much drowned.

The Tertiary Coast Ranges stretch from Puget Sound southward, and since they were formed by Middle and Late Cenozoic orogeny it is probably impossible to discern the extent of uplift of the continental margin or the fall in sea level. Raised beaches are prominent in many places and indicate Quaternary uplift.

The Coast Ranges of California are similar to those of Alaska, inasmuch as Tertiary sediments and orogeny are involved in both. Yet the Alaskan Coast Ranges are much embayed, from Yakutat Bay through Prince William Sound and Cook Inlet to Kodiak Island, whereas the Coast Ranges of California with a few exceptions present rather bold straight shorelines, lacking embayments. Tertiary sediments have been elevated in the Coast Ranges of Alaska several thousand feet above sea level, yet the ranges are extensively embayed. The writer looks upon this contradictory situation as one in which the orogeny and consequent structures developed independently of sea level, and with rising sea level in Alaska the deformed strata became somewhat drowned whereas with falling sea level in California the deformation developed an extensive emergent terrain.

Conclusion: North America, if considered relative to a uniform sea level, thus appears to have tilted downward to the north and upward to the south, with the hinge line extending from Lat. 42°N on the east to Lat. 48°N on the west. It is here postulated, however, that sea level has changed, having risen in the polar regions and fallen in the equatorial regions. Reasons will be pointed out later.

Epeiric movements inside the continent have also occurred, particularly the late Cenozoic uplift of the Great Plains, Colorado Plateau, and Rocky Mountains. These crustal movements range up to 8000 ft in magnitude, and are believed to stem from activity in the mantle. Along the Atlantic coast the vertical movements of land relative to sea level are undoubtedly separate in origin from the intracontinental movements. The Cenozoic belts of orogeny along the Pacific coast do not yield as clear a picture of relative and progressive land–sea movements as that of the Atlantic.

It should be noted that the hinge line between emergence and submergence on the Atlantic has shifted southward in late Cenozoic time, for originally it was at Long Island or northward (Lat. 42°N) and is now as far south as Cape Hatteras (Lat. 35°N). The embayment at the mouth of the Columbia River may indicate a southward shift on the Pacific coast.

South America

West Coast: The Andean mountain complex is one of uplift and emergence from the Isthmus of Panama to Lat. 40°S; from this latitude southward through Tierra del Fuego the orogenic system becomes progres-

sively drowned. There are undoubtedly a number of local exceptions to the generalized statement, but the broad picture is clear (see Figure 1).

East Coast: The Brazilian Shield was almost entirely below sea level in Late Cretaceous time and has since become land. Cretaceous deposits veneer Precambrian rocks extensively from the equator to Lat. 24°S. A very narrow coastal plain and continental shelf occur, and the coastline is generally bold and unembayed like a coast of emergence. Some geologic event in pre-Cretaceous time, perhaps rifting, has left this margin unusually abrupt, and so with uplift little coastal plain has been produced.

The first major embayment is the Rio de la Plata at Lat. 30°S, and from this place southward the continental shelf becomes very broad and the continental margin much submerged. The extremely narrow shelf of the low latitudes, in fact, begins to broaden off Rio de Janeiro (Lat. 23°S), where it is about 80 miles wide. At Lat. 38°S it is about 150 miles wide, and at Lat. 45°S it is 300 miles wide. The hinge point between uplift and submergence as well as can be told on the east coast of South America is at about Lat. 35°S and on the west coast at 40°S.

The depths along the outer margin of the continental shelves of South America, plotted on Figure 2, show fairly well a deepening toward the south.

Arctic Region

A glance at the geologic map of the Arctic published by the Alberta Society of Petroleum Geologists (1960) shows strikingly the drowned, embayed nature of the continental margins around the Arctic Ocean basin. The outer margin of the shelf lies at depths of 1200 to 1350 ft from Greenland across Spitzbergen and Novaya Zemlya, as well as can be discerned from U.S. Navy Hydrographic charts. This is approximately the depth of the outer margin of the Antarctic shelf. Broad submerged valleys in the shelf between Spitzbergen and Novaya Zemlya reach depths at the outer shelf margin of about 1800 ft.

The Alaskan Arctic Coastal Plain and Mackenzie delta are exceptions, for there the outer margin of the shelf lies only 150 to 500 ft deep. The Alaskan Coastal Plain is an emergent region, principally since the Cretaceous. Tectonically, it is a continuation of the Great Plains province of the United States and Canada, and may have been responsive to the same causes of epeiric uplift in late Cenozoic time, and thus an exception to the widespread submergence of the rest of the Arctic region. The Mackenzie delta, like other river-built margins, is exceptional, and its profiles are not useful for the decipherment of sea-level movements.

The shelf beneath the Chukchi and East Siberian seas is rather shallow, but reliable data for the depth of the outer margin of the shelf were not

found. Perhaps this region is a continuation of the Arctic Coastal Plain of Alaska.

Antarctica

Continental Shelf: The continental shelf around Antarctica ranges in width from 25 to 200 miles and averages approximately 50 miles. The maximum width of the shelf across the Ross Sea is 400 miles, and the Berkner Bank in the Weddell Sea is 300 miles broad. The outer edge of the shelf is marked by the 1800-ft isobath on Atlas Plate 65 of the National Geographic Society map of 1963. Soundings under the Ross Ice Shelf range from 1476 to 3030 ft. Woollard (1962) reports a depth of about 400 meters (1300 ft) for the continental shelf, presumably its outer edge.

Since the continental ice wedges out at the shoreline or becomes buoyant in the ocean waters, isostatic adjustment of the continental shelf would not extend many miles outward, and the outer margin of a shelf 50 miles broad would not be much affected. Its present depth should mark the pre-ice depth corrected to eustatic sea-level changes incident to glacial stages.

Pre-Ice Topography: If the continent of Antarctica were free of ice, there would be an upward adjustment of 3000 ft at Byrd Station and 3300 ft at the Pole of Inaccessibility (Woollard, 1962). A profile by Woollard showing the amount of upward adjustment that would occur from Byrd Station to the Ross Sea indicates that the region was mostly land in pre-ice time.

In the same basin or trough as Byrd Station, but toward the Bellinghausen Sea, is a place where the bedrock floor has been sounded 8000 ft below sea level and where the ice is 14,000 ft thick. Upward adjustment here would be about 4700 ft, and the rock surface would still be over 3000 ft below sea level. It may therefore be judged that the island festoon of Palmer Peninsula would lead to a much embayed region between the Ross, Weddell, Bellinghausen, and Amundsen seas. Behrendt (1963) concludes that the rugged, high southern part of the Antarctic Peninsula would be separated from the Sentinel Range, and that the Ross and Weddell seas would be connected even after complete isostatic rebound (Figure 1). Although the pre-ice topography is not yet well defined, this part of Antarctica appears to be one in which sea level has risen, much like that of South America south of 30°S Lat.

The coastal region between the Amery Ice Shelf and Lützow-Holm Bay is probably a large island bridged by ice to the mainland. In one place at least the bedrock floor is 850 ft below sea level, but since the ice is 7000 to 8500 ft thick, an upward adjustment would undoubtedly provide land connections with the large island. Still an embayed coastline would probably exist.

From consideration of the depth of the continental shelf around Antarctica, and the pre-ice topography of parts of the continent, it is concluded that sea level has risen 600 to 800 ft. Since the Andean mountain system extends into part of this embayed region, and since the southern Andean system has become partly submerged in Tertiary time, it is thus expected that the rise in sea level around Antarctica occurred since the Cretaceous.

Africa

Coastal Plains: The sea has withdrawn in Libya and Egypt, leaving a broad embayment and coastal plain of Upper Cretaceous, Tertiary, and Quaternary sediments. Along both sides of the Red Sea are narrow coastal plains of Miocene sediments now elevated 600 to 700 ft above the sea. The large triangular peninsula of British Somaliland, Somalia, and eastern Ethiopia (Lat. 5–10°N) is an emergent region of marine Cretaceous and Tertiary sediments. A coastal plain stretches continuously from Lat. 2°S to 28°S, in which Upper Cretaceous and Tertiary sediments have emerged (Pepper and Everhart, 1963). As the available maps do not show topography, the amount of uplift here is unknown.

The west coast of Africa from Lat. 10°N to Lat. 13°S is almost continuously an elevated coastal plain, involving Cretaceous and Tertiary sediments in much the same manner as the eastern coast. Uplifted local deposits of Cretaceous sediments mark the southern coast from Cape Town to Port Elizabeth at Lat. 34°S, where the older Hercynian mountain belt exists. At places along the coast, Precambrian rock or continental Tertiary deposits are flanked by very narrow shelves and a steep continental margin. This condition seems consistent with uplift of the land relative to sea level as indicated by the extensive coastal plains.

Continental Shelves: The continental shelves around Africa are generally narrow; the continental margin is regular and abrupt, with little tilt or gradient and a general depth of 600 to 700 ft. This would be expected, according to the postulate of fall of sea level in the equatorial latitudes.

Australia

The western part of Australia is cratonic and fairly stable, much like that of central and southern Africa. Although the north coast facing the Timor Sea is much embayed, the west coast is fairly regular, with an exposed border of Jurassic, Cretaceous, and Cenozoic sediments. This condition attests to a preponderance of uplift over subsidence of the continental margin in Cenozoic time. Raised beaches of Pleistocene age are extensively developed. (See the geologic map of Australia by David [1931], and new map of the Indian Ocean by Pepper and Everhart

[1963].) The coast becomes embayed starting at about Lat. 35°S according to Carrigy and Fairbridge (1954), and the southern tip of continental Australia, as shown by the Bass Strait (Lat. 38°S) and Tasmania, is much embayed and drowned without a coastal plain of any sort. The change from general elevation of the continental margin to submergence thus takes place at Lat. 35°S, and approximately corresponds in position to the same change in South America.

The western Australian shelf ranges in width from 24 to 250 miles and is marked by inner and outer terraces. The outer one generally has a steeper gradient, and its edge ranges in depth from 60 to 110 fathoms (Carrigy and Fairbridge, 1954).

This western Australian shelf is the site of accumulation of large quantities of clastic carbonate material. Primary terrigenous sediment is largely confined to those sectors having high rainfall and strong tidal flow. The origin of the shelf differs from place to place but is generally a combination of erosion and sedimentation. The Sahul shelf (Timor Sea) is sinking, due probably to tectonic activity, and the Rowley section of the west coast is probably being locally downwarped as judged by a negative gravity anomaly and an outer shelf margin of 300 fathoms in one place (Carrigy and Fairbridge, 1954). Pleistocene eustatic sea-level changes are clear, especially along the north coast.

Meaning of Depth of Shelf Margin

There must be considerable question in the reader's mind of the meaning of the depth of the outer shelf margin, but the evidence that it is generally shallow in low latitudes and becomes progressively deeper toward high latitudes is fairly substantial. The data here presented have been collected for relatively stable coasts mostly from U.S. Navy Hydrographic charts. Echo sounding profiles would be highly desirable, and eventually must be gathered and studied. Reflection profiles that reveal the structure of the shelves point to a complex and partly puzzling origin, particularly of those that are nondeltaic. The recent article by Moore and Curray (1963) presents two subbottom reflection profiles, one off Norfolk, Virginia, and one off Newport, Rhode Island. The shelf or "continental terrace" in both places is analyzed as one of upbuilding and outbuilding during the Cenozoic. In the intervening area off the Hudson River, Ewing and others (1963) describe considerable prograding or outbuilding of the shelf slope (called the Hudson apron) during the Wisconsin epoch, while sea level was much lower than now and most of the shelf was exposed. During this time the Hudson River eroded a broad valley across the shelf; in Recent time this valley has been mostly filled. A submerged terrace at a depth of 500 ft marks the maximum fall of sea level, and Ewing and co-workers believe that terraces on the Argentine shelf at 360 to 450 ft in depth, and on the shelf of the

northwest Gulf of Mexico at 440 ft, mark the same shoreline. A fluctuating sea level during the Pleistocene, of the same order of magnitude as the shift in sea level from low latitudes to high, as here proposed, may prove confusing. The polar shift is viewed as a slow process, the Pleistocene fluctuations as a superposed influence.

The Norfolk section of the continental shelf must be viewed in relation to the adjacent emerged Coastal Plain. If the entire margin of the continent, including both shelf and Coastal Plain, were flooded at the beginning of the Cenozoic, sea level stood relatively about 500 ft higher than now. Thus the outer margin of the shelf, as it now exists, would have been possibly under 2000 ft of water. General sediment upbuilding and sea-level fall have combined to reduce the depth of the margin to about 600 ft.

Some shorelines, and the known sediments that build up the adjacent shelves, point to a late Cenozoic origin, and it may be difficult to think of any shelf that has endured without major alteration since the Cretaceous. Yet the progressive deepening toward high latitudes suggests a rising sea level with which sedimentary accretion could not keep pace, and the complementary emergent coastal plains in low latitudes convey evidence that the process started at the close of Cretaceous time.

Decrease in Earth's Period of Rotation

Astronomical observations over the past 200 years leave little doubt that the length of the day has increased, but bothersome irregularities have evoked much study. Ancient eclipses and occultations suggest that the increase can be measured for the last 20 centuries. Munk and Macdonald (1960) have critically discussed the many astronomical and geophysical problems connected with attempts to measure the rate and variation of increase of length of day, and arrive at a fractional rate of change of 2.1×10^{-10} per annum. The slowing-down of the earth's rotation is now generally agreed to be due mostly to oceanic tidal friction, although other possible causes have been proposed, such as expansion of the earth and shift in mass due to ice-cap formation.

In the theories of evolution of the earth-moon system the slowing rate of earth rotation has been extrapolated backward. With the rate proposed by Munk and Macdonald it will be seen that 100 million years ago the earth was rotating about $1/50$ faster than now. If a change of this order is sufficient to have caused a shift of the oceans toward the poles in the magnitude of about 600 ft of rise in sea level, it will have to be determined by individuals better versed in mathematics than the author. The presumption, of course, is that the solid earth has not responded completely to the slowing rate of rotation, but that the oceans have.

Mathematical considerations of the lag of the solid earth in adjusting to a decreased rate of rotation may lead to a better understanding of the

physical properties of the crust and mantle, and it is hoped that the geological data here presented are worthy of such study. No doubt the relations of response of oceans and solid earth to slowing rotation are complicated and will need careful investigation.

Historical Changes in Sea Level

Changes in sea level as recorded by tide gauges over the past century might be examined for evidence of the postulated rise in the polar regions and fall in the equatorial, but the undertaking is soon found to be without value. Gutenberg (1941) and Kuenen (1950) conclude that sea level has risen around the world in amounts ranging from 10 to 20 centimeters in the past century. Study of Gutenberg's figures and the map of Munk and Revelle (1952) reveal no special trend in relation to latitude. Reversals are common at single stations, local crustal movements are recorded at others, and long-term barometric changes are known to have affected the sea level. More significant still is the response of sea level to ice-cap buildup and melting, a rate and amount that would entirely mask any small change produced by slowing of the earth's rotating during the Pleistocene.

Conclusions

There has been a fall in sea level in the low latitudes and a rise in the high latitudes since the Cretaceous. The amount of fall in the equatorial regions is about 600 feet, and the amount of rise in the polar regions is probably greater than 600 feet. These data may help determine the rate of slowing of rotational velocity of the earth, and also yield something about the strength of the earth. The gradual change in land and sea areas may have brought on the ice age.

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PERSPECTIVES

PRESENTATION OF DR. HUGH STOTT TAYLOR AS THE WINNER OF THE 1964 WILLIAM PROCTER PRIZE

The Associate Editors have taken it upon themselves to include President Harvey A. Neville's introductory remarks when he presented the William Procter Award to the Editor-in-Chief on October 6, 1964.

It is indeed a privilege and a pleasure to present Hugh Stott Taylor for the William Procter Prize, awarded for scientific achievement by the Scientific Research Society of America, the non-academic research affiliate of the Society of the Sigma Xi.

It is always pleasant to participate in giving away, especially to a most deserving recipient, that which has been provided by others at no personal cost. This gives one a feeling of benevolence and power, like that exercised by a director of a charitable foundation or an agency of the Federal Government.

For the present recipient, Dr. Taylor, I have enjoyed this pleasure on several occasions which, for my own satisfaction and with your indulgence, I shall recount:

I presented him to receive the Franklin Medal, the highest award of the Franklin Institute of Philadelphia, in 1957.

In June of 1961 I presented him with an honorary doctoral degree from Lehigh University, to add to the many he already had, when he was our commencement speaker.

Upon his retirement as Dean of the Graduate School of Princeton University in 1958, I presented him with his portrait on behalf of the Association of Princeton Graduate Alumni—an autonomous organization not to be confused with the general Alumni Association of Princeton University. I should add that immediately after giving this portrait to Sir Hugh, I retrieved it by previous agreement and presented it to President Goheen for Princeton University, where it now hangs in Procter Hall at the Graduate College.

A year or two after leaving Princeton for my first position on the chemistry staff of the University of Illinois, where I was secretary of the local section of the American Chemical Society, I arranged a speaking tour for my former professor, Dr. Taylor, at several midwestern sections of the Society. When he arrived at Illinois, I proudly showed him the manuscripts of two research papers which I had ready to send to the editor of the Journal of Physical Chemistry. He patiently read these papers, and his only comment was: "Harvey, you have two split infinitives in these papers." If I had split an atom, he would, no doubt, have commended that early discovery of fission. Of course, I found and repaired the infinitives and have never committed this error since—except when I undertake to intentionally and emphatically split one. At the time, I was naturally somewhat crestfallen by my mentor's strictly negative criticism of my research effort, but in retrospect I think perhaps his comment was high praise, coming from a then rather reserved Englishman; for if he had found reason to disapprove the scientific content of these papers, he would certainly have said so. I'm sure you will all be relieved to learn that the articles were published as amended.

These opportunities I have had to participate in a few of the many occasions when

Hugh Taylor has been accorded recognition and high honors, in no substantial degree meet my obligation and indebtedness to him. Upon his return to Princeton in 1919, after serving the British Government in scientific work during the First World War, I came under his influence as a teacher and the director of my graduate research. The phrase "under his influence" suggests a degree of intoxication, and I think this accurately describes the state of those of us who have had the privilege of extended association with him. I consider myself one of his early experiments in catalysis, and his activating effect has been felt over all these years.

Hugh Stott Taylor was born in St. Helens, England, and educated at Liverpool University, receiving there the D.Sc. degree in physical chemistry in 1914. His graduate study also included a year at the Nobel Institute, Stockholm, Sweden, as Exhibition Fellow, and a year at the Technische Hochschule, in Hanover, Germany.

He came to this country in 1914 to join the faculty of Princeton where he was advanced to the rank of Professor of Chemistry in 1922, served as Chairman of the Department of Chemistry from 1926 to 1951, and as Dean of the Graduate School from 1945 to 1958. He was made Dean Emeritus in 1958 and none remarked that this was an honor which he should have received earlier.

Dean Taylor is a member or fellow of numerous scientific and honorary societies in this and other countries. He was President of the Society of the Sigma Xi in 1951–52 and is presently Editor-in-Chief of its journal, AMERICAN SCIENTIST. He was President of the Faraday Society in 1952–53 and President of Pax Romana from 1952 to 1955. He is currently President of the Woodrow Wilson National Fellowship Foundation.

In recognition of his scientific accomplishments, and as evidence of his truly international reputation, Dr. Taylor has been awarded honorary degrees by many colleges and universities throughout the United States and various other countries. He was presented with the Nichols Medal of the American Chemical Society in 1928, the Mendel Medal by Villanova College in 1933, the Research Plaque by the Research Corporation of New York in 1939, the Franklin Medal of the American Philosophical Society in 1941, the Longstaff Medal of the Chemical Society of London in 1942. In 1953, Queen Elizabeth II of England made him Knight Commander of the Order of the British Empire and, at Vatican City, Pope Pius XII made him Knight Commander of the Order of St. Gregory the Great.

The research activities of Dr. Taylor have been in the field of physical chemistry, especially in basic studies of the kinetics, mechanism and catalysis of chemical reactions at solid surfaces. This work began during World War I with Sir Eric Rideal and resulted in a book, "Catalysis in Theory and Practice," under their joint authorship, published in 1919. This was the first significant book on the subject of catalysis. In current books on catalysis the theoretical part is much expanded, largely as the result of studies made by Dr. Taylor and his co-workers. In the first phase of these studies, the Langmuir concept of adsorption was tested and used to explain the rates of catalyzed reactions in heterogeneous systems. Chemisorption was distinguished from physical adsorption by the magnitudes of the heats of adsorption determined for numerous solid catalysts of varying degrees of activity.

In the second phase of these studies, Dr. Taylor developed and presented, first to the Royal Society, the concept of the heterogeneity of catalyst surfaces. This was a most significant advance in the theory of contact catalysis. Through this means Dr. Taylor was able to interpret the relative efficiency of catalysts, the effects of poisons and promoters, and the activation energetics of catalyzed reactons. In the third phase of his work, Dr. Taylor utilized the newer methods of surface area measurements to further the development of catalytic theory, and employed the deuterium isotope to trace more precisely reactions occurring at the surface of solid catalysts.

The scientific papers of Dr. Taylor, published in various European and American

journals, number more than two hundred and fifty. He is also the author, part-author, or editor of thirteen books, principally in the field of physical chemistry.

Undoubtedly a major accomplishment of Dr. Taylor has been in his capacity as an educator, in his stimulating guidance and encouragement of many students and co-workers who, in turn, have made and continue to make significant contributions to science. Because of his own scientific investigations and his influence upon other scientists of this generation, he ranks as one of the most distinguished chemists of the world in our time.

Mr. Chairman, in recognition of his many notable contributions to the science of physical chemistry; his long and distinguished service to higher education; and his eminent achievements as author, editor and teacher, I have the honor of presenting to you to receive the William Procter Prize, Hugh Stott Taylor of Princeton, New Jersey.

I do not know what our "Lend-Lease" arrangements with England have been, but the services of Sir Hugh Taylor to science and education in the United States have more than repaid us for any aid we have given to the mother country.

ACADEMIA AND INDUSTRY—THEIR MUTUAL INFLUENCE

By HUGH TAYLOR

> "Let men look ahead to a time when scientific efforts will be combined and then artfully distributed, one man taking charge of one thing and another of another, working together in their labors and industries. In that day, said Bacon, scientists will begin to show their strength."
>
> —CATHERINE DRINKER BOWEN
> "Francis Bacon—The Temper of a Man"*

THE ESTABLISHMENT of the Scientific Research Society of America some sixteen years ago and its generous encouragement and endowment by Mr. William Procter is significant of the changes that have been occurring during the present century in the relations between science originating in academic institutions and the researches, in ever-increasing volume, from industrial centers. Two decades ago, the Society of the Sigma Xi, while rapidly expanding its activities in Chapters and Clubs, was forced to recognize that there were "Companions in Zealous Research," of merit and distinction, who had never had the opportunity of election to Sigma Xi but who, from the nature of their present occupations and the character of their scientific discoveries, were well qualified to share in the encouragement of scientific research, the principal objective of the Sigma Xi Society. In the colleges and universities of the country, the parent Society had attained a prestige, paralleling that of the older Phi Beta Kappa Society, but limited to professors and students in the scientific fields. In order not to alter the academic background

of the Sigma Xi organization it was decided, rightly I believe, to establish a sister society with similar objectives but operating in the industrial and government research centers of the land. At the last Annual Convention, in December 1963, it was reported that RESA now has eighty-five branches installed or authorized, a membership of about 11,000, and an endowment with a market value of some $200,000. For a young lady in her teens, RESA, may we call her a daughter society of Sigma Xi, with such a splendid dowry, gives a very attractive and impressive promise of adulthood.

Tonight, we wish to gain some perspectives on and insight into the mutual relations between academia and industry, private and governmental, as they have developed since the industrial revolution initiated the rate of change in scientific development. That revolution has become, in these latter decades, a scientific revolution, of chain characteristics, resulting in an explosive expansion of technology, an intrusion into the lives of everyone, and a scope extending to the outermost regions of space and time. Alexander sighed for new worlds to conquer. Science and technology have not only explored the moon, but have received and understood messages about events that occurred before man was.

Catalysis

It is convenient to use as a launching pad for an excursion into the roles of academia and industry the subject of catalysis and catalytic reactions. It was the researches in the first decades of the nineteenth century, largely in university or institutional centers, which uncovered the activity of finely divided platinum in a variety of oxidation processes. Edmund Davy, Dobereiner, Sir Humphry Davy, Erman, Turner, Henry, and Faraday are all associated with this activity. Dulong and Thenard in France showed that gold, silver, and even glass shared the same property if the temperature of these agencies was sufficiently raised. Faraday's researches provided insights into the mechanism of catalytic change and into the causes of "poisoning," contributions that remained unmatched well into the 20th century. Berzelius, in 1835, coordinated a number of isolated observations and outlined his ideas as to a "catalytic force" which Liebig regarded as superfluous, substituting an hypothesis of "molecular vibrations," unassailable because it could not be submitted to experimental test.

A Bristol vinegar merchant, Phillips, in 1831, was the first to attempt to use platinum industrially for the oxidation of sulfur dioxide. His material rapidly poisoned and the process was abandoned. Squire and Messel, in 1875, made the process a technical success using platinum catalysts to convert sulfur dioxide from pure sulfuric acid into "oleum," sulfuric acid containing dissolved sulfur trioxide. The industrial production of "oleum" by the contact sulfuric acid process was the achieve-

ment of German industrialists at the end of the nineteenth century, who solved the problem of removing poisons from the sulfur dioxide obtained from pyrites, and thus paved the way for the massive development of the dyestuff industry, with synthetic indigo as the initial industrial target.

The honors in the field of hydrogenation and dehydrogenation go to Sabatier and his co-workers in the University Laboratories of Toulouse. Nitrogen fixation to yield ammonia stemmed from the classical researches of Haber in Karlsruhe and Berlin. The Badische Anilin u. Soda Fabrik had transformed the thermodynamic and equilibrium studies of Haber on a variety of catalysts to a technical process, ready in May 1914, an achievement which freed Germany from dependence on Chile saltpetre and led ultimately to the penetration of synthetic ammonia into the explosive and fertilizer industries of the whole world. In 1925, the same German technical organization adapted the discoveries of Sabatier to the production of synthetic methanol on oxide catalysts, the hydrogenation-dehydrogenation characteristics of which Sabatier had given ample proof. His studies of metal hydrogenation catalysts, such as nickel, led directly to the industrial development of fat-hardening.

Major basic scientific contributions to the study of reactions at surfaces came from the work of Langmuir in the laboratories of the General Electric Company at Schenectady, work which provided research activities far and wide in university centers such as Cambridge, Bristol, and Princeton in the 1920's. During the same period, the Fixed Nitrogen Research Laboratory in the U.S. Department of Agriculture entered the field of basic research on the synthesis of ammonia. Doubly-promoted catalysts, slow sorption of nitrogen as a rate-determining step, and the definition of surface area by the B.E.T. method are among the outstanding contributions from government laboratories to this field.

Reviewing the whole story, one has to conclude that academia provided the initial impetus, the continued injection of new ideas and concepts. Industry contributed magnificently to technical development and government laboratories supplied much that neither industry nor the academics had found.

Were we to extend this survey to the developments of the last thirty years, especially in the petrochemical field, in the production of polymeric materials, rubbers, and plastics, the same pattern of interactions could be observed.

Radio Astronomy

Just over thirty years ago one of the newest sciences was born, as a direct result of communications research in the laboratories of the Bell Telephone Company. When Karl Jansky set up, in Holmdel, N.J. in the fall of 1930, a 14.6-meter rotatable antenna and its associated receiving

and recording equipment, he listened to static from local thunderstorms, static from thunderstorms some distance away and a third group "composed of a very steady hiss static the origin of which is not yet known." In April 1933, at a meeting of the International Scientific Radio Union, Karl Jansky presented a paper on "Electric Disturbances Apparently of Extra-terrestrial Origin." We recognize the scientific modesty behind the use of the word "Apparently" and we know that Jansky was receiving his messages from a large elliptical area which is aligned with the general direction of the Milky Way. [1]

We have been witnesses to the birth and development of a new science that originated in an industrial laboratory. Within one generation the new science had penetrated the universities and discovery followed discovery with bewildering speed. The reach of our senses has been enormously magnified. Radio stars were found beyond the limits of the astronomical instruments. Exploding stars, galaxies in collision, spiral arms in our local galaxy, signals from neighboring galaxies, and from the Sun, Jupiter, and Venus, powerful radio-emitters have been found. Around the world, parabolic reflectors are focusing signals not only from the outermost regions of space but also from the efforts of man in satellites around the earth and expeditions around and on to the Moon and other planets. Jodrell Bank monitored Ranger VII until the moon passed below the horizon just before impact. Higher resolutions and much greater information can be obtained by using a radio-cross. The Astronomy Center of the University of Sydney is now building a large cross-type radio-telescope at the Malonglo Radio Observatory some twenty miles from Canberra. The arms of the cross are approximately one mile in length and forty feet wide. At present the East-West arm has been completed. The North-South arm poses more difficult technical problems and, hopefully, will be complete within another year. Radiation has been detected over a small waveband at 21 cm, an emission line produced by the neutral hydrogen atom, concentrated within the spiral arms of our galaxy, and in other galaxies besides our own. "Studies in radio astronomy range in wave length from perhaps 1 cm to about 30,000 cm. . .this is roughly 12 octaves. Previously the wave length range available to the astronomer extended from about 3000 to perhaps 30,000 Å, or roughly 4 octaves. Between the two lies a vast region of about 12 octaves[1]." Southworth summarizes, in conclusion, the achievements of the radio astronomer in these words:

> "Following Reber's very creditable start at mapping the radio heavens, the astronomer is now filling in a substantial amount of detail. With his new tools he is having quite as much of a field day as did Galileo more than three centuries ago when, with his newly invented telescope, he discovered in rather rapid succession, the moons of Jupiter, the rings of Saturn, and the dark spots of the sun's disk as well as certain mountainous features of the moon."

Nuclear Science

The development of nuclear science has been confined almost exclusively to academia from its beginning in the last decade of the nineteenth century to the time when Fermi and his group first demonstrated the existence of a nuclear chain reaction.

The discovery of X rays by Roentgen in 1895 was followed rapidly by Becquerel's discovery of radioactivity and by the demonstrations of the existence of the electron in the last five years of the nineteenth century. In these same years, the Curies were revealing the presence of radium and of polonium in separations from pitchblende. Rutherford showed the complex nature of the radiations, the first two components, the alpha- and beta-rays having different penetrating powers. Pierre Curie found a radiation which was not deflected in magnetic fields and it became the gamma-radiation. By 1908, Rutherford and Royds had proved that the alpha-particles were helium nuclei, which became the probes for the study of atomic structure. Alpha-particle scattering led Rutherford in 1911 to suggest the modern picture of the nuclear atom. Soddy, Russell and Fajans were studying the radioactive elements and their transformations from which resulted, in 1913, the concept of isotopes. In the same year, J. J. Thomson demonstrated the existence of such isotopes in non-radioactive elements, a domain which Aston richly explored from 1919 onwards. That was the year, also, in which Rutherford, bombarding nitrogen with alpha-particles, changed nitrogen nuclei into oxygen nuclei.

Researches by Bothe and Becker in Germany, and by the Joliots in Paris were the preliminaries to the proposal by Chadwick in 1932 of a new radiation consisting of uncharged particles with the approximate mass of the proton. The existence of neutrons was experimentally established and became the newest tool in the production of new isotopic and radioåctive species because of its ability to penetrate the charged nuclei of atoms. The development of particle accelerators by Van de Graaff, Cockcroft and Walton, and by Lawrence placed other tools at the disposal of the scientist for the exploration of the nucleus and its transformations. Einstein's statement of the generalized equivalence of mass and energy provided the quantitative bases for the energies required for or produced by nuclear transformations. It was, however, the interaction between neutrons and uranium nuclei, initiated by Fermi in 1934, which culminated in the observations of Hahn and Strassman, in 1939, that the products of interaction were nuclei approximately one half of the mass of the original nucleus, one of the products being barium. Meitner and Frisch, in Copenhagen, realized the significance of the observations and were aware of the immense energy that could be released in the fission. This energy release was confirmed immediately in many laboratories throughout the scientific world. It led to the demonstration of the nuclear chain reaction by Fermi and his group on Decem-

ber 2, 1942, "in secret, in a war laboratory, heavily financed by the United States" as H. D. Smyth observed [2]. Nuclear science passed out of the hands of the academicians; industry and governments took over. To continue, in the words of Smyth:

"In many respects the discovery of uranium fission marks the end of an era in scientific research. It was truly international, it was made by small groups working on a small scale, for the most part in university laboratories, and it was made in the atmosphere of freedom and frankness that had meant so much to science. It remains to be seen how fully we can return to such conditions."

Waves and Particles: In 1913, Niels Bohr published his work on the hydrogen atom, with the electron in stationary states revolving around the positive nucleus, the proton, absorption and radiation involving quanta, $h\nu = E_2 - E_1$, ν being the frequency, h was Planck's constant, and the energies in the two states being E_1 and E_2. The experiments of Franck and Hertz verified the prediction of discrete energy levels in processes involving collisions of electrons with atoms but attempts to extend Bohr's ideas to more complex nuclei than hydrogen presented increasing difficulties met empirically with more and more quantum numbers until, finally, they gave way to the new quantum mechanics of Schroedinger and Heisenberg in 1925–26. By 1930, the application of the new principles to the electronic structure of atoms and their chemical behavior was well advanced.

It was Louis de Broglie, in 1924, who paved the way for the new wave and quantum mechanics. He reversed the customary mode of thought concerning the atomistic discrete structure hitherto characteristic of matter. As with radiation, with wave properties and quanta, de Broglie proposed that material particles could also possess the wave nature of light. This development is pertinent to our present theme, since confirmation of the wave nature of electrons was obtained in 1927 by Davisson and Germer in an industrial research laboratory with a study of the reflection of electrons from a single crystal of nickel. G. P. Thomson, in the same year, provided the additional evidence required, by demonstrating that a stream of accelerated electrons, passing through thin films of metal produced characteristic diffraction patterns, the radius of the diffraction ring being proportional to the wave length λ of the electrons diffracted, a wave length given by the de Broglie relationship,

$$\lambda = h/mv$$

with a particle of mass m and velocity v, h being again Planck's constant. Thomson has recently generously pointed out that the earlier work of Davisson in industry led him to his experiments in academia.

Tools and Techniques in Scientific Research

It would be impossible in the course of one address to survey the respective contributions of university, government, and industrial

research in all branches of scientific effort. As an alternative, one can summarize the tools and techniques that are employed in modern science and show how great is the mutual influence of academic and industrial science in these areas.

A typical story in such development is to be found in the area of photosynthesis. Fifty years ago, an examination question on this topic could be adequately answered by a basic overall equation:

$$CO_2 + H_2O \xrightarrow{h\nu} O_2 + (CH_2O)_n.$$

The examinee would receive high marks if he put $h\nu$ over the arrow connecting reactants and products. How research in the intervening years has progressed to the point that Calvin could receive the Nobel Prize in Chemistry in 1961 for his researches in this field can be well illustrated by an excerpt from a recent book by Calvin and Bassham on this subject. Figure 3 in their book reproduces the results of a radioautograph of a two-dimensional paper chromatogram of an alcoholic extract of the alga, *Chlorella pyrenoidosa*, after 10 seconds of photosynthesis with radioactive carbon dioxide $C^{14}O_2$. Identified in the chromatogram are alanine, malic acid, aspartic acid, phosphoenolpyruvic acid, triase phosphates, 3-phosphoglyceric acid, sugar phosphates, and sugar diphosphates. The experiment involved the development of chromatography, the use of radioactive isotopes as a tracer tool and autoradiography to determine the extent and mode of entry of radioactive carbon into the photosynthetic cycle. As long ago emphasized by Newton, the new scientist surveys farther horizons from the shoulders of those who have preceded him.

A random selection of advances over the years can be cited in illustration of tools and techniques stemming from major discoveries in science for which industry provided superb equipment for use in future research. We can think of Svedberg's centrifuges and their development for molecular weight determinations of large molecules; of Irving Langmuir's utilization of the mercury vapor pump to produce high vacua; of Urey's discovery of deuterium and his no less important formulation of isotope separation involving zeropoint energy differences, which gave us heavy nitrogen N_2^{15}; of Tiselius' electrophoresis apparatus; of Martin and Synge demonstrating the principles of chromatography with their instrumental developments, with columns, paper, and thin film chromatographs; of Bridgman's high pressure techniques that combined with temperatures reaching to 9000°F. for brief intervals, or to 5000°F. for hours, resulted in the production of industrial Man-Made diamonds by the General Electric Company. Progress in solid state physics and semiconductors came largely from the industrial laboratories, and the findings of Shockley, Brattain, and Bardeen in the laboratories of the Bell Tele-

phone Company gave the world the transistor and its manifold applications in all areas of science and life. Libby's technique of carbon dating gave a new tool to archeological and allied geochronometrical studies. Cockcroft, Walton, and E. O. Lawrence did the pioneering work which has led to the development of the multibillion electron volt machines now in use in the exploration of the nucleus. Powell's observation of high energy particle tracks in emulsions and Glaser's bubble-counters are essential complements to such work in the nuclear field. The Mossbauer effect now takes its place among those yielding instruments for the elucidation of chemical structures, notably in the newly investigated noble gas compounds. Nor must one overlook the more mundane spectroscopic instruments which in the infrared, visible and ultraviolet provide invaluable facilities for the elucidation of complex organic structures. Instrument makers have contributed overwhelmingly to the rapid advances of modern science both in the universities and industry.

I forbear, for reasons of colossal ignorance, to say anything about the computer revolution now twenty years old. In the Bell Laboratories Computing Centers, for example, the demands for computing services have grown exponentially since about 1950, doubling in about 13 to 16 months. While leadership in computer science and technology has now reverted to the industrial laboratory there are memorable names from academia associated with the development. One thinks initially of the English logician R. Turing in 1927, of Vannevar Bush and his differential analyzer, of Howard Aiken at Harvard and his computing machines, of John von Neumann, his interest in ENIAC in Philadelphia and JONIAC at the Institute for Advanced Study and his posthumous book "The Computer and the Brain." Industrial research "has now accomplished some of von Neumann's most daring aspirations, such as the design of computers by computers, the virtually complete solution of the mechanization of algebra, etc." [3].

Still more recent is the development of masers and lasers, now a bare ten years from conception, with academic origins through the studies of Townes, of Weber at the University of Maryland and in Russia [3]. The patent of Schawlow and Townes, granted to them at the Bell Laboratories and embodying their concepts as published in the *Physical Review* in December 1958 opened up the whole domain of optical masers and lasers, the development and application of which in science and in industry is now in full cry.

A Personal Glance Backward

In 1917, Captain E. K. Rideal had been withdrawn from behind the Somme trenches where daily he had dosed water supplies with chlorine for the troops in his area. In the Munitions Inventions Department in

London we were studying the catalytic conversion of water gas and steam to give a cheaper hydrogen supply. We used a flue-gas analyzer for carbon dioxide which metered the gas supply, absorbed the carbon dioxide in alkali, and then by re-metering gave the CO_2 content as a difference. At the request of the Royal Air Force, we adapted this machine to the determination of oxygen in hydrogen by catalytically converting to water and measuring the volume change. Still later, we devised a continuous recorder for carbon monoxide in hydrogen by preferential conversion of the monoxide to dioxide, absorption in lime water, and determination of the change in electrical conductivity. Subsequently, in 1922, with Guy B. Taylor at the duPont Company, this method of gas analysis was generalized with a machine which dosed gas samples and absorbing liquid in suitable amounts. We began our report of this work with the statement that "Chemical industry has been singularly backward in encouraging the development of automatic devices for control of operations."

In 1921, Dr. R. M. Burns initiated the Princeton program on adsorption of gases by catalytic materials. To obtain his vacuum he used a Toepler vacuum pump, with a device of Professor G. A. Hulett substituting a water pump for the laborious task of raising and lowering kilograms of mercury. Langmuir mercury vapor vacuum pumps do not appear in the record of Princeton adsorption studies before 1927, when Kistiakowsky used them in his work on adsorption by methanol catalysts.

Harvey Neville, at a bench adjacent to Burns, was studying the interaction of steam and carbon as catalyzed by alkali carbonates. He, also, was indebted to Professor Hulett for his method of dosage with steam. This was generated by a heated wire immersed in water heated externally with steam, the electrical input to the wire determining the steam produced. I am afraid that gas analyses were made by hand-operated Orsat apparatus.

Early efforts to discover the nature of the absorbed species on catalysts were made by Gauger using electron collisions with hydrogen adsorbed on nickel and by Wolfenden and Kistiakowsky measuring ionization potentials, the former with adsorbed hydrogen on nickel, the latter with nitrogen adsorbed on iron. Kistiakowsky attributed an 11-volt ionization potential to adsorbed nitrogen and a 13-volt potential to adsorbed hydrogen. He argued that the results indicated adsorbed atoms rather than molecules.

In a further effort to ascertain the properties of adsorbed hydrogen, studies were conducted on atomic hydrogen produced at room temperatures by photosensitization with excited mercury. Here it may be emphasized that it was necessary to build the sources of resonance radiation, one interesting form being built from two quartz to glass seals, the quartz

ends fused together and the glass ends becoming G-702 P glass cathode and anode compartments respectively.

When the para-hydrogen conversion was invoked to indicate activated adsorption of hydrogen on various catalysts it was necessary for Sherman to build his own thermal conductivity cell. Still more so, when deuterium was used as an isotopic tracer in exchange reactions it was necessary to use ultraviolet spectroscopy as an analytical tool for deuteroammonias and deuterobenzenes and to adapt infrared spectra for analytical purposes for the measurement of deuteromethanes and alkanes. Soon, a mass spectrometer became indispensable and one was built from Nier's blue prints. There were no infrared or mass spectrograph machines to purchase at that time. It was well into the post-World War II period, when the most active mass spectrographic work in Princeton was over, that the first Consolidated Mass Spectrograph was purchased for Frick Chemical Laboratory.

It is interesting to note how many and varied techniques have been employed since those early days for the exploration of adsorption by catalytic materials. One thinks of Selwood's various essays in the field of magnetochemistry, and of proton relaxation and catalyst accessibility; of Turkevich's studies in electron microscopy and small angle X-ray scattering; of Eischen's studies of infrared spectra of adsorbed species with, now, an extended bibliography from centers throughout the scientific world; of Beeck's techniques of thin films of catalyst metals which, since his death, have yielded a rich harvest to Kemball, Tompkins and others; of radioactive tracers, principally of tritium and carbon-14 which have helped to delineate the catalyst surface; of gas chromatography which permitted Emmett to perform, in minutes, reactant gas analyses which, in the 1920's, would have occupied days, or permitted Beebe and others to explore by alternative techniques the basic problem of adsorption equilibrium; nuclear magnetic resonance and electron spin and paramagnetic resonance have been brought into play; semiconductivity and catalysis by compounds and d-band holes in metals and alloys have led to electronic interpretation of catalysis. All this magnificent development in just one area of scientific research has been made possible largely by the contributions of industrially developed instrumentation in the postwar period and has enormously accelerated the pace of progress. This is probably the most significant impact of industry on academia in scientific research.

I end on a personal note. In 1925, by a deductive leap in the dark, I suggested that catalyst surfaces could not be homogeneous, that from the heterogeneity might stem some of the most characteristic properties of catalytic materials. Active centers became the focus of argument "about it and about." There were no tools then to force a decision. But, in the

intervening years, as Ehrlich of the General Electric Research Laboratory [4] pointed out a year ago:

"The outstanding feature of the past decade has been the development and perfection of a variety of experimental techniques that allow a new and deeper insight into the structure of surfaces, and into the elementary atomic events occurring on them . . . What is important is that experiments properly executed and interpreted, can now remove from the realm of speculation most of the elementary facts dealing with interplay of gases and an initially clean surface. This tremendous advance has, in large measure, been made possible by the reduction of ultrahigh vacuum techniques to a matter of complete routine."

Muller's field ion microscope resolves the details of the atomic arrangement of the surface and Ehrlich has shown that direct observation of adatoms is indeed possible.

W. O. Baker [3] has informed me, on this same topic, that low energy electron diffraction studies conducted by Germer and MacRae at the Bell Telephone Laboratories have explicitly confirmed the prediction of activated surfaces and that, in the same laboratory, Lander has shown differences in the spacings and thus in the valence bonding of silicon atoms on especially prepared cleaved surfaces of the element, employing also low energy electron diffraction. Industry thus makes academic dreams come true.

The Future

What does this peering into the past tell us about the future. Long ago I said that the mantle of the prophet lies uneasily on the scientist. I. I. Rabi in an article in the *New Scientist* [5] expressed it more pungently. "It is a characteristic of scientists in general that they have no flair for predicting the future. That is better done," he wrote "by the H. G. Wellses and Aldous Huxleys."

We can, however, indicate trends in academic, industrial, and governmental relations which will surely emerge in the coming decades. We can be certain that the present exponential growth in science that has characterized the post-World War II years will continue until economic and other factors produce a developmental plateau. The relations between academia and industry will become more intimate and interrelated. We can expect the lag between discovery and application which formerly was some twenty years, but which has shortened so dramatically in recent times, may well be further abbreviated. The explosive increase in the number of scientists makes certain that the tempo of application will remain high.

It has been in the universities that the "seed corn" of technological progress has often been raised during the past century. This makes it imperative that, in the universities, there exist a climate favorable to

the production of basic science from which technological applications inexorably result. That climate requires leisure time to think on the part of first-rank scientists, unhampered by undue calls on their services as consultants to industrial and governmental agencies, to the maximum degree possible. Such scientists must be free to roam where their scientific spirit moves them, again unhampered by calls for the solution of particular scientific and industrial problems. To that end, we might well consider a major extension of governmental, foundational and industrial financial support to key scientists in the universities across the land to pursue *their own* scientific objectives, without regard to particularized contractual obligations and requiring only the reports on their endeavors that emerge in their scientific publications. By way of example, one can note the annual Reports on Researches, published by Research Professors, Research Fellows, and Research Students, that appear annually in the Year Books of the Royal Society of London. Endowments approaching $5.5 millions permit expenditures by the Royal Society in excess of $200,000 per year for such purposes. In addition, the Society administers Parliamentary Grants for Research Professorships and scientific investigations, exceeding $350,000 in the year 1963–64. The researches so supported are characterized by their basic scientific interest and their freedom from anything other than personal choice. These choices range from molecular biology to X-ray spectroscopy and radio astronomy, from Mendelian populations to high energy physics and satellite launching, from cavitation to geochronometry.

Industry on the eastern and western seaboards of this continent have already demonstrated the immense significance of the pursuit of basic science in their own industrial research laboratories, and its fruitfulness. Until a similar condition obtains in the areas between these rich scientific and technological concentrations, the potentialities of the country are not being fully realized. To attain progress in these central areas consideration might well be given to the establishment of research institutes now in successful operation in the South, the South West, and in Stanford, to name just a few, where a central research institute, competently staffed by skilled personnel, can count on the support and cooperation of interested industries. For the scientifically underdeveloped areas of the country some consideration might be given to plans for and probable future of such research institutes. A current example from Canada has been recently discussed by Dr. A. D. Misener, Director of the Ontario Research Foundation [6], a plan to strengthen Canada's technology, to lessen Canada's reliance on foreign technology, and to halt the "brain drain" which has resulted from the lack of research opportunities. Within a year, a versatile research community should be started on a 339-acre site near Toronto. The Foundation will occupy

100 acres of the plot. The remaining acreage is to be allocated to industrial companies that wish to participate. The International Nickel Company of Canada, Consolidated Mining and Smelting Company. Dunlop International Research and British American Oil Company have agreed to join and purchase land. The Province of Ontario is giving both financial assistance to and encouragement of the project. Sheridan Park will constitute a challenging enterprise which might well be emulated in areas south of the Canadian border.

The gross national product in the United States has been rising by 25 to 30 billion dollars annually in recent years, and will soon surpass, if it has not already passed, the 600 billion dollar mark. What fraction of this huge productive effort should be returned for the further support of science and technology, with which academia and industry may ensure the future progress of the country, is a question which should engage the serious consideration of all who are qualified to contribute to the answer. It will be an important answer for the welfare of the country.

Acknowledgment

In accepting the William Procter Prize for 1964 I wish to acknowledge all the help, kindness, and consideration from students, colleagues and friends that alone have made this award possible.

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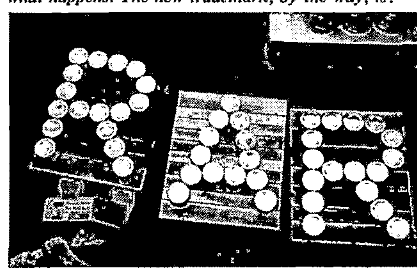

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Silazanation, if the truth were known, drifted into our ken in the late summer of '63. A chemical news story told that a team at a medical school in Pittsburgh was having great good luck purifying carbohydrates and related polyhydroxy compounds by GLC with the trimethylsilyl derivatives. Without further ado we turned to the bench and enjoyed the same good luck with it on our vitamins and monoglycerides. Others had spectacularly good luck in *their* fields. Papers are popping all over on GLC of silazanized fatty acids, steroids, plant sterols, bile acids, alkaloids, barbiturates. There has also been one (*Ann. 659*, 190) on silazanation for peptide synthesis. The best is yet to be, now that we announce the easy availability of EASTMAN 9151.

And to whom are we all indebted? In part, to the U. S. Congress for voting tax money to help the coal industry compete in liquid fuels. In carrying out this mandate of the people, the U. S. Bureau of Mines found trimethylsilyl derivatives useful in analysis, identification, and purification of phenols and pointed out the applicability of GLC to the problem. The whole bit, including catalysis of the reaction by a little chlorotrimethylsilane and explanation of why this works, comes from the Bureau. True, the Bureau was probably being influenced by organosilicon research sponsored by a glass company and two other big companies that happen to make silicones. A man from one of the latter had gone so far as to suggest that trialkylsilyl derivatives of alcohols and phenols are low-viscosity liquids easy to prepare, thermally stable, resistant to air oxidation, and easy to hydrolyze.

There is also another use for EASTMAN 9151 in GLC. It inactivates the support materials against adsorption of the gaseous solute in competition with the stationary-phase liquid. This was discovered in the non-political, non-commercial, prestige-covered halls of Cambridge University, alma mater to Isaac Newton. The boys were grinding up firebrick and taking their lead from the U. S. Bureau of Mines on the affinity of hexamethyldisilazane for the hydroxyls in the stuff.

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THE SCIENTISTS' BOOKSHELF

By Hugh Taylor, the Associate Editors, and Guest Reviewers

SEE INDEX AT END OF THIS SECTION

Vitamins & Coenzymes by ARTHUR F. WAGNER & KARL FOLKERS; 532 pages; $17.50; John Wiley & Sons, Interscience, 1964.

The authors of this up-to-date volume have succeeded in summarizing a wealth of knowledge. The topics covered are thiamine, riboflavine, nicotinic acid, pantothenic acid, pteroylmonoglutamic acid, biotin, pyridoxine, lipoic acid, choline, *meso*-inositol, *p*-aminobenzoic acid, essential fatty acids, and vitamins A, B_{12}, C, D, E, and K. Where appropriate, the coenzyme derived from a particular vitamin is thoroughly discussed in the same chapter as the vitamin.

In general, each chapter discusses the isolation of a particular vitamin, the determination of its structure, its synthesis, its biosynthesis, its metabolic (or coenzyme) role, the mechanism of coenzyme action (where known), and its nutritional and therapeutic role. The book is easy reading and is profusely illustrated with structural formulae. Extensive documentation with references to the original literature is provided. Excellent author and subject indexes are present as well as a glossary of the more obscure vitamin terms with which one must still struggle, especially when reading the earlier literature.

Unfortunately, the high price of *Vitamins and Coenzymes* will make it unavailable to the average student; however, serious nutritionists, scientists working in the natural products field, and biochemists will find the book indispensable.—*Jacques Dreyfuss*

Brain Mechanisms. Progress in Brain Research: Vol. 1; edited by G. MORUZZI, *et al.;* 493 pages; $25; American Elsevier Publishing Co., 1964.

This book is a collection of research reports and their discussion by 34 leading brain-research scientists, presented at a meeting of the International Brain Research Organization in Pisa, Italy, 1961.

Because this book is a collection of individual research reports, it would not be very suitable as a textbook of brain physiology. On the other hand, the book has great value to the research worker in this field, because it presents some elegant research and discussions. In fact, the discussions of each report by the participants in the conference are among the most stimulating aspects of this volume. Together with the reports themselves, the comments provide a most exciting intellectual atmosphere for researchers who seek new ideas.

Many of the original research results reported are coupled with rather extensive reviews of the recent literature on the respective subjects (50 percent of the references in 5 randomly selected papers were dated from 1959 to 1963). Some of the general subjects discussed, along with new findings, are: central inhibitory functions, homeostasis of the reticular formation, integration at the thalamic level, evoked responses, and mechanisms of sleep. Although this volume is not strictly comparable to the now classical neurophysiology volumes of the Handbook of Physiology (1960), it is a valuable supplement. Perhaps most significant of all is the publisher's declared intention to publish several such volumes each year, which will present recent advances and stimulate further research. Such a continuous updating of progress in the field would be especially valuable because brain research is presently in its most creative period. Indeed, brain research is today one of the great frontiers in science, even though it is at least temporarily overshadowed by the exploration of space.—*W. R. Klemm*

Advances in Chemical Physics, Vol. V, edited by I. PRIGOGINE; 410 pages; \$16.50; John Wiley & Sons, Interscience, 1963.

A review volume of this sort comes as a mild shock to someone with a solid-state physics type of background. The standard series in this field, such as Seitz and Turnbull's "Solid State Physics" or Gibson's "Progress in Semiconductors" demand of the reader some knowledge of the Schroedinger equation and its approximate solutions in a reasonable number of different kinds of Brillouin Zones by way of background. The book considered here assumes a much higher level of preparation and gives the feeling of having been written by one group of specialists for the benefit of another. For example, three of the eight articles require a good grounding in group theory, a subject which is not in the repertoire of a great many physical chemists or experimental physicists. Two others assume a background in statistical mechanics at the level of the papers currently appearing in the "theoretical" section of the Physical Review.

The only article which the general reader has a fighting chance to cope with is the fascinating discussion entitled "Convex Molecules in Gaseous and Crystalline States" by Taro Kiharo of the University of Tokyo. His purpose is to establish a quantitative definition of the concept of convexity and then show how this information enables one to predict the geometrical properties of a light spherical gas using data from diffusion experiments. The effective cross-section for collision can be expressed in terms of three fundamental measures, using the three Steiner relations of differential geometry. These measures for a sphere, for example, are the volume, the surface area, and the quantity $4\pi a$, where a is the radius. These fundamental measures when combined with diffusion data taken at 0 °C. enable one to make a remarkably good estimate of the size and shape of heavy molecules. For example, CO_2 is found to have a rod-like shape about 4 x 8 Å and benzene is a hexagon about 6 Å across. These ideas are also applied to a discussion of the cohesive energies of molecular crystals.

The remarks made above are not meant to be criticisms of the material in this book, since the reputation of the editor should be sufficient guarantee of the quality of the individual pieces. They are merely meant to serve as a guide to the reader.—*Allen Nussbaum*

African Ecology & Human Evolution, edited by F. CLARK HOWELL & FRANCOIS BOURLIERE; 666 pages; \$12.50; Aldine Publishing Co., 1963.

Classification & Human Evolution, edited by SHERWOOD L. WASHBURN; 371 pages; \$7.50; Aldine Publishing Co., 1963.

Exactly one century before the publication of these volumes Thomas H. Huxley pleaded the evolutionists' case for man's descent. The contrasts between Huxley's slim *Man's Place in Nature* and the books considered here are sufficient to provide me with an excuse for a personal centennial celebration. In 1863, primate behavior was known from highly imaginative travelers' accounts and the fossil evidence for human evolution would have filled no more than a few hat boxes. A hundred years later, twenty primate species had been reliably studied in their natural habitat and parts of close to one thousand hominids had been unearthed. A celebration is surely in order: the past century has produced the evidence that Huxley did not have.

The crucial evidence, however, has raised interpretive and classificatory problems. These problems are the subject matter of the seventeen contributions to *Classification and Human Evolution*. In the area of interpretation, two emphases, evident throughout the book, underscore and extend important trends in the study of human evolution. First, there is an emphasis on thinking of fossils as samples of populations. Second, stress is placed on the study of relevant morphological complexes evolving at different rates. Both of these trends have come late in the study of man where every scrap of bone takes on peculiar importance. In the area of classification, one apparent consensus leaps out against a background of differences that are mild by comparison. Two

genera and only two genera of fossil hominids are recognized—*Australopithecus* and *Homo*. The latter includes under a single generic roof all of those finds known popularly as Heidelberg Man (actually a lower jaw), Swanscombe Man (parts of a skull and more probably a lady), Java Man (really the bones of a number of hominids), the Neanderthal population, and a host of others. It is clear in this volume that classification is no longer confined to gross comparison; studies of chromosomes and hemoglobins take their place along with approaches that would have been familiar to T. H. Huxley.

It was not Huxley, but Darwin who in 1871 (*The Descent of Man*) wrote "... it is somewhat more probable that our early progenitors lived on the African continent than elsewhere. But it is useless to speculate on this subject." The proposed location of our "early progenitors" has changed from time to time since 1871 as evidence pointed first to one continent and then to another. Because recent finds now support Darwin's projection, anthropological eyes are refocused on Africa.

The work of twenty-three contributors, *African Ecology and Human Evolution* is largely a study of change in that vast continent during the Pleistocene. Specialties that range from ornithology to geomorphology are represented and every major region of Africa is given some coverage. The book is an exemplar of an approach that views the evolution of man within the framework of his total environment. This is the appropriate framework, for, when one thinks of early man, one must imagine an animal that stood in so intimate a relationship with his environment that he cannot be understood apart from it.

The result of ecological studies are useful in understanding man's descent only if they are sufficiently local. In the past, the African Pleistocene has been reconstructed by analogy to high latitude phenomena. The concentration here on local conditions, with less recourse to analogy yields the kind of information that anthropologists value. To cite one of many possible examples, the conclusion that the contrast between wet and dry seasons in east Africa was greater than the contrast between pluvials and interpluvials helps one to understand persistence and change in the abundant fossil hominids from that region.

In *African Ecology and Human Evolution* I read that, in both southern and eastern Africa, "The scenery of today was the scenery of our earliest Pleistocene ancestors, and erosion during this time has merely put the finishing touches to our present landscape" (p. 330). It is thus unfortunate that little attention is devoted to the ecology of contemporary human hunters and gatherers in that region while much is made of the behavior of baboons and gorillas. *Classification and Human Evolution* is open to a similar critique. Above all, human evolution is a process in which genetics and tradition play complementary roles. Yet nowhere do I find a section on the classificatory potential of traditional human behavior congealed in the matter men shaped into tools. But again, there are a few chapters on non-human primate behavior. Future volumes in this field would benefit by more liberally heeding the human facets of human evolution.—*Robert Ascher*

Selected Problems in Physics with Answers by M. P. Shaskol'skaya & I. A. El'tsin; 246 pages; $3.75 paper; The Macmillan Co., Pergamon, 1963.

A formal description of this book would include the information that it consists of an English translation from the Russian of three hundred and seventeen problems and their answers, arranged under eleven subject headings (Kinematics; Statics; Electricity and so on) at the level, the authors write in their foreword, "not ... confined within the limits of the secondary-school syllabus." In American, the problems are appropriate for freshman physics students taking a course that omits the use of calculus, and would be useful also to students whose course includes calculus, as well as to bright high school students.

Thus far, the book's description would almost fit "Worked Examples in

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MATHEMATICS • PHYSICS • CHEMISTRY • BIOLOGICAL SCIENCES • MEDICAL RESEARCH • SPACE SCIENCES • ENGINEERING • PSYCHOLOGY

Physics" by Zubor and Shal'nov, published in 1962 by Pergamon, containing a few more problems and at a somewhat higher price. However, the book here being reviewed, unlike the earlier one and the reviewer's own experiences in college physics, stresses less problems whose solutions consist of choosing a correct formula and substituting in it appropriate numerical values, but instead contains many questions to be answered by a few thoughtful sentences. Opening the book to an arbitrary page, I give as an example problem number 75; "Will the work and power expended by the motor of a moving stairway change if a passenger standing on it as it moves upward, himself walks up the the staircase at a constant speed?"

I have checked a number of the problems and have located only one with a wrong answer. Problem number 186 asks why, when one finishes stirring a cup of tea, the contents come to rest with the tea leaves piled up in the center of the cup. The correct answer is that, as friction with the cup walls and bottom slows down the rotation near these surfaces, centrifugal force at the bottom becomes less than that higher up in the liquid. A secondary rotation occurs in which liquid rises at the center of the cup, moves radially outward at the top surface, downward along the cup sides, and radially inward at the bottom. This last carries the tea leaves toward the center. This answer was given by Einstein in an essay on "The Cause of the Formation of Meanders in the Courses of Rivers," in "Essays in Science," Philosophical Library, 1934. Considering that Einstein thought the problem obscure and interesting enough to discuss it, I cannot find it in me to condemn very strongly anyone else who fails to see the correct solution, particularly since it was the only error I could locate in the present book.—*Herbert Malamud*

Recent Progress in Microcalorimetry by E. CALVET & H. PRAT; 177 pages; $8.50; The Macmillan Co., Pergamon Press, 1963.

As progress in science continues, the questions that beg solution demand concomitant progress in technology. More detailed knowledge is sought regarding the functions of physical, chemical, and biological systems and the magnitudes of the physical quantities to be measured extend to ever more extreme values. Thus, instrumentation must become more sensitive, more precise, more stable over increasing periods of time, etc. The measurement of heat produced by physical, chemical, and biological systems is important as it yields information regarding their structure and function.

The book under review deals with such an instrument, viz., the Tian-Calvet microcalorimeter, which is useful, for example, in following the heat output of a single seed as it germinates and starts to grow, and in the study of very slow chemical reactions such as occur in the hardening of cement. Parts I and II, by Calvet, deal, respectively, with the theory and some design considerations of this instrument and descriptions of some general physico-chemical applications such as measurement of specific heats, thermal conductivity, heats of solution and chemical reaction, and studies of catalytic activity. While the discussions of Part I identify important points for consideration, it would appear to be virtually impossible to design and construct a practical instrument from the contents of these pages alone. For such information, the reader must refer to the treatise by the same authors published in 1956. Part III, by Prat, is a series of brief descriptions of the application of this type of microcalorimeter to studies involving biological structures. Included here are studies of the heat production during seed germination, bacterial growth, and effects produced thereon by antibiotics, insect metamorphosis, and thermogenesis of reptiles and infant mammals.

The bibliography deals almost exclusively with the extensive work of the authors and many references are made to the 1956 publication, "Microcalorimétrie."—*Floyd Dunn*

Mandelstam Theory & Regge Poles by R. Omnès & M. Froissart; 123 pages; $7.50 cloth; $3.95 paper; W. A. Benjamin, Inc., 1963.

In the past few years, since the successful application of dispersion relations to forward pion-nucleon scattering, issues involving the analytic structure of scattering amplitudes have become a dominant theme of theoretical particle physics. More and more variables have moved successively into the complex plan: first, energy (ordinary dispersion relations); then, momentum transfer as well (the Mandelstam representation); and most recently, angular momentum (Regge poles). Any actual confrontation with experiment must of course take place for real, physical values of the scattering variables. But excursions into the complex plane have provided new insights, and, in particular, have opened up the possibility of exploiting more fully the general principles of unitarity and crossing symmetry.

Two related questions are involved here. One concerns the analytic structure itself; the other has to do with the observable implications of this structure. The simplest propositions about the analytic properties of scattering amplitudes can in fact be derived from the general principles of quantum field theory (ordinary dispersion relations for certain reactions are a case in point). For the rest, one turns to an analysis of Feynman graphs and of non-relativistic potential scattering as sources of conjectures or for support of conjectures arrived at in other ways.

The excellent monograph under review, written by two eminent French physicists, devotes about half its space to a review of analyticity questions for potential scattering. Since they are addressing themselves to experimentalists, the authors rightly eschew rigor and concentrate on heuristic and physical discussion. In the bargain, they provide an elegant and lucid review of scattering theory generally; and the discussion successfully illuminates many of the issues that are at stake for the field-theoretic problem of actual physical interest.

The second half of the book is concerned with the field-theoretic problem: the kinematics, the notion of crossing symmetry, Mandelstam's analyticity conjectures and their implications; and the Regge pole hypothesis. The very last chapter considers some of the implications of the Regge hypothesis for scattering at high energies.—*S. B. Treiman*

Osmotic & Ionic Regulation in Animals by W. T. W. Potts & G. Parry; 424 pages; $9; The Macmillan Co., Pergamon, 1964.

Here is a book as refreshing as a breeze. It deals with an area of biology where publications have often tended to be encyclopedic presentations of scattered and somewhat unrelated data. While such compendia have been not only important but essential, a different kind of approach is highly welcome. This work demonstrates that the developments of the past decade have brought the field to the point where more unity is discernible. The material is presented clearly, effectively, and interestingly.

This is a book which will be useful not only for the worker or potential worker in the field, but will also be good reading for the biologist whose primary interests are elsewhere. The presentation is carefully adapted to such a reader, attempting to bring unity to scattered information, to analyze and interpret the data, to weigh controversial issues, to point out both areas of knowledge and points of ignorance. If in a few places the authors fall a little short of this aim, their average of success is high.

The writers make no pretense to a comprehensive tome. Minor details are frequently sacrificed for clarity and for principle. However, rich leads to the literature are provided by nearly 700 references. The fact that these are made by title is a welcome feature; and if this adds somewhat to the cost of publication it is an investment which pays big dividends in usefulness.

The general range and the foci of the monograph are indicated by the major areas with which it deals: general aspects of osmoregulation including dis-

cussion and definitions which introduce the uninitiated into this field; excretory organs, vertebrate and invertebrate, including protonephridia, contractile vacuoles, and Malpighian tubes as well as ultrafiltration systems; ionic regulation in marine animals; osmotic regulation in brackish, fresh-water, and land-living animals; hypo-osmotic regulators, and electrolyte metabolism.

This work is a summary of a physiological field; it is a summary of one aspect of ecology; it is a summary of some factors involved in animal distribution. It is recommended both for the general biological reader and for the actual and potential worker in any of the sub-divisions of the field covered by the monograph.—*Rudolf T. Kempton*

An Introduction to Waves, Rays & Radiation in Plasma Media by J. J. BRANDSTATTER; 690 pages; $15; McGraw-Hill Book Co., 1963.

One is reluctant to point out any specific failings of this book, lest the inference be drawn that the author's treatment is otherwise adequate. In actual fact, this is not a book at all, but a set of lecture notes haphazardly edited. Perhaps this may explain the cumbersome notation and the stylistic blunders, e.g., "They are called the Stokes-Navier-Stokes equations after the men who first derived them." However, nothing could extenuate the terribly misplaced emphasis: all too often we are drawn through tedious "derivations" leading nowhere, while the essentials of the topic receive scarce mention. An extreme, but by no means isolated, example of this is the author's treatment of an inhomogeneous medium (Chapter VI). After a long bout with complicated variational forms and an excursion into functional analysis, the only conclusion reached is that "there is no royal road to the solution of problems involving inhomogeneities." Of course, there is a royal road (the WKB method), but it lies hidden in a problem in the appendix.

Unfortunately, it must be said that the entire book is characterized by such misplaced emphasis. In all but one of the nine chapters the author actually treats the plasma only in the restricted sense of a magneto-ionic fluid whose dielectric properties are obtained from a zero temperature model incorporating a phenomenological collision frequency. While such an approach may often be useful, in ionospheric problems for example, the basic problems of electromagnetic plasma behavior are accessible only when the plasma is treated statistically. However, the one chapter (Chapter VIII) in which a microscopic viewpoint is adopted, is completely superficial. The author performs some involved manipulations on the Fokker-Planck form of the collision integral, then suddenly abandons the program in favor of the original zero-temperature model.

This book is advertised as a text, and its defects are magnified as sins of pedagogy. The imbalance toward waves in a zero-temperature plasma is so complete that such phenomena as bremsstrahlung, microinstabilities, synchrotron radiation, and ion-acoustic waves are not even mentioned. Landau damping is neglected because it "requires the use of complex variable theory!" But the student, reading this "text," deserves to learn what plasma actually is, not what the author may think it is or wish it to be.

It is upsetting to see a book like this appear. Badly written, badly edited, printed by photo-offset from a typewritten manuscript and done up in a cheap binding, it sells for $15. Perhaps enough institutions will purchase it to make similar ventures profitable. If that turns out to be the case, should the publishers be absolved of responsibility? Something is wrong somewhere.—*Fred Shure*

The Psittacosis Group as Bacteria by J. W. MOULDER; 95 pages; $4; John Wiley & Sons, 1964.

This monograph, the sixth in the series of CIBA lectures in microbial biochemistry, is a concise survey of the biology and biochemistry of the Psittacosis group of organisms.

In the first two chapters the morphology, growth cycle, chemical

composition, and metabolic properties of the organisms are outlined. These chapters are liberally illustrated with tables, graphs, and electron micrographs; unfortunately, several of the latter are poorly reproduced. The third chapter is devoted to summarizing Professor Moulder's ideas on the relationship of these organisms to bacteria, and their classification as energy parasites. A useful set of recent references has been included for each chapter.

The systematic presentation of some aspects of Psittacosis biochemistry and the indications of gaps in our current knowledge of these organisms will, perhaps, whet the interest of those working in related fields.

According to the information presented on the book's jacket, the book is designed to convince one that the Psittacosis agents are not large viruses but small bacteria or at least very close relatives of bacteria and, further, that there is an absolute distinction between viruses and other infectious agents. With respect to the latter point one might argue that the apparent gap between viruses and bacteria merely reflects our limited knowledge of the details of replication of even well-known intracellular parasites. As for the demonstration that Psittacosis agents are not viruses, the arguments are convincing and the information apparently timely; the inclusion of at least one paper on these agents in a well-known journal ostensibly devoted to virology, suggests that virologists, at least, would profit by reading the monograph.—*B. R. McAuslan*

Earthquakes and Earth Structure by JOHN H. HODGSON; 166 pages; $3.95; Prentice-Hall, 1964.

This extremely readable book portrays in graphic style the meaning and experience of earthquakes. By a judicious selection of a few important quakes—several well known and some more obscure—the author has managed to explain the origin, recording, and interpretation of quakes and to examine possible methods of avoiding "the scourge of the earthquake."

The layman's interest in these natural phenomena is mounting continuously. Each new major quake arouses curiosity and fear about the forces at work within the earth. *Earthquakes and Earth Structure* explains some of the "facts, effects, and fancies" of earth tremors. The author does not hesitate to mix qualitative and quantitative treatment; an equation or two can aid in clarifying such concepts as the relationship of energy to earthquake magnitudes and the computing of distances and thicknesses from travel-time curves.

Many implications about the earth's structure are drawn from the initial examination of what earthquakes are like and how seismologists study them. Possible causes of earthquakes are analyzed in terms of current theories and information obtained by an analysis of the statistical distribution of quakes in time and space.

Well-written and pleasingly illustrated, this volume is relatively free of error in its presentation. Although intended principally for the "non-specialist," the book should prove to be a handy addition to the library of many students of the earth.—*Marvin E. Kauffman*

Handbook of Physiology, Section 2: Circulation; Vol. II, P. DOW, Executive Editor; W. F. HAMILTON, Section Editor; 1786 pages; $32; Distributed for the American Physiological Society by The Williams & Wilkins Co., 1963.

Five years ago, when the first volume of this series made its appearance, the publication of this American version of the German *Handbücher* prompted two questions. One of these concerned for whom the book was written; the other related to the price tag. These questions may still obtain in the case of the present volume.

In truth, any criticism of the present volume probably applies to the series as a whole. There is some uneveness which is inevitable in multiauthored books, but it is of minor importance. The chapter on the role of endocrines and stress, for example, seems less than

exhaustive, and one would have liked a longer discussion of some aspects of neonatal circulation, such as, closure of the ductus arteriosus. There is an abundance of clinical material, but I think not a preponderance. Also, in the rapidly moving areas such as renal physiology, some material, understandably, may not be current.

These criticisms have always applied to textbooks of physiology. *The Handbook of Physiology, Circulation*, however, is no ordinary textbook. It is a super-text in which basic concepts are assumed. It is a high quality, advanced work. Moreover, it is a super-text with a rich reference list, and good index. These latter qualities make it a most welcome effort to the instructor as well as to the graduate student. This then is the answer to the first question.

It is expensive. I think that in my graduate student days, I would have had to use the library copy. In these days of affluence, however, it would even seem modestly priced compared with other publications. Frankly, if you are an instructor in physiology or a rich graduate student you cannot afford not to have it.—*John W. Bauman, Jr.*

The Mystery of the Expanding Universe by W. Bonnor; 212 pages; $7.50; The Macmillan Co., 1964.

Many of the essential procedures and results of modern cosmology have been put into non-mathematical form readily accessible to the layman in this book. The author, Professor of Mathematics at the University of London, first orients the reader by briefly discussing the nature and magnitude of planets, stars, galaxies, and the cosmological unit—clusters of galaxies. This section includes 12 photographs, mainly of galaxies and red-shift spectra similar to those found in many elementary astronomy texts.

The second section describes the methods and difficulties of the observational astronomer and the types of data needed for the study of cosmology. A short, though not superficial, discussion leads into the theoretical part.

It is this third part which is best written and most exciting. Attention is given to the meaning of models in physics and the philosophical outlook of the working cosmologist. The author introduces the happy pedagogical device of distinguishing between cosmological models by their expansion curves, graphs of the size of a model universe as a function of its age. Emphasis is placed upon the relativistic and steady-state models, but this book also contains the only simple account known to the reviewer of Gödel's model of a rotating universe. About a dozen models are described briefly but in sufficient detail to help the reader judge their relative historical, philosophical, and scientific positions.

Finally the most recent observations, the Hoyle-Ryle controversy, and the author's conclusions regarding the merits of relativistic and steady-state theories are reported. The text is delightfully opinionated throughout, but the author is careful to state where facts leave off and speculations begin. Except for an occasional minor technical inaccuracy (e.g., discounting the relevance of the velocity for which time travel is possible in Gödel's model), the volume is well-written and highly recommendable as an introductory survey. Though binding and typography are good, the price seems rather too high for a non-technical book of this nature.—*William C. Saslaw*

Germfree Life & Gnotobiology by Thomas D. Luckey; 512 pages; $17.50; Academic Press, 1963.

The word *gnotobiology* is used for the first time in this book and is defined by the author as "the study of, or knowledge of, organisms living under such conditions that all species in intimate contact with each other are known to the investigator in absence of all other demonstrable viable organisms." It would seem that this new word would find a permanent place in the investigator's vocabulary as it clearly defines the situation and lends itself to such adaptations as *gnotophore*, indicating an organism existing in intimate contact

with one or more known species and no other demonstrable viable organism, *dibiote*, an organism living in intimate contact with only one other, *tribiote*, etc. Dr. Luckey also coins other words which are given in a glossary but which are reasonably definitive, and in general self-explanatory.

One of the most amazing things in this book is the presentation of historical material showing that germfree life antedates by many years the work done at Notre Dame. This must come as a surprise to workers other than those intimately engaged in the field. Most people, when asked, will state that the production and use of germfree animals started with Dr. Reyniers and Lobund.

The book has six chapters: Theory and General Aspects of Germfree Life and Gnotobiology, Phylogenetic Development of Germfree Research Germfree Animal Techniques, Nutrition of Germfree Animals, Characteristics of Germfree Animals, and Gnotophoric Animals. The last two chapters emphasize the difference in the conception of the germfree and gnotophoric states. At the end of the book, in addition to the Glossary, there is a chronology, and eight tables of diets for various types of experimental animals.

The book is well written and directions for producing and maintaining colonies of germfree life for insects, fish, poulty, rats, large animals, etc., are comprehensive, and explicit. Nearly a thousand references make the volume particularly useful.

It is, perhaps, unfortunate that more space was not given to the work of Dubos and Schaedler but since their work was with *polybiotes* this may be an unfair observation. Certainly, the book is fascinating and will be of value to all biologists whether active investigators in the field or just interested on-lookers. —*David B. Sabine*

The Planet Mercury by W. SANDER, translated by A. HELM; 94 pages; $3.95; The Macmillan Co., 1963.

There are many possible audiences for a book about a planet, including the professional astronomers, amateur astronomers, the general public, the astronautics community, science fiction writers and fans, children, etc. The author of this little book does not specify his intended audience; however, it appears to be aimed at the amateur.

The highly specialized professional astronomer may feel that all the well-established data on Mercury could be summarized in only a few pages and that there is, therefore, no need for a book such as this. However, the teacher of astronomy may be interested in the history of observations and speculations concerning the atmosphere and surface, while the amateur should also be interested in the chapter on practical observations.

It is unfortunate that the book was written just too early to miss the very interesting observations and conclusions of N. A. Kozyrev on the atmosphere of Mercury. His spectrographic studies show an atmosphere of hydrogen which he calculates could be sustained by influx of solar protons.

If Kozyrev's results are confirmed, Mercury will become much more interesting as a target for instrument probes and even manned space flights. Even the very thin atmosphere, which he estimates at 0.003 of the earth's, should be sufficient for atmospheric braking. This would make the difference between a very difficult astronautic target—more difficult than Jupiter—and one only slightly more difficult than Mars and Venus.

Dr. Sandner was apparently not writing for the astronautics community since he says very little about the prospects of travel to Mercury. Additional material on this subject could be generated which would quickly fill a longer book than the present one. However, until this has been done, Dr. Sander's book should prove useful for the growing group of scientists, engineers, students, and amateurs who are taking an increased interest in the innermost planet.—*Dandridge M. Cole*

The Ecology of North America by VICTOR E. SHELFORD; 610 pages; $10; University of Illinois Press, 1963.

This treatise is the culmination of the

life work of Victor E. Shelford, the "father of animals' ecology and bioecology." It is a weighty volume comprising 19 chapters: an introduction covering the scope and meaning of ecology and the remaining chapters devoted to the major biotic areas of North America. These include, among others, the temperate deciduous forest, the floodplain, boreal coniferous forest, montane coniferous forest, tundra, cold desert and semidesert, grassland, hot desert, tropical rain forest, cloud forest, and communities of southern Florida and Cuba.

Over 800 references are incorporated, thus bringing together an overwhelming number of ecological studies. The pattern of organization for each major biotic unit is first, a general description of the region as a whole, followed by a discussion of the associated vegetation and animal populations including dominants, influents, and permeants.

According to the fly leaf statement "This book is the first comprehensive description of North America from an ecological viewpoint as it appeared in the period 1500–1600." Obviously this is a difficult but worthy project and an effort is made to touch upon this period occasionally. However, the work is primarily an invaluable compilation of descriptive vegetation, animal ecology, and bioecological research of the last half century.

In such an undertaking omissions are inevitable. For example, Bromley's excellent monograph on the *Original Forest Types of Southern New England* and, for the Southwest, Humphrey's review of the desert grassland before the arrival of white man and his cattle are omitted. On the whole, the animal coverage appears superior to that of the vegetation.

The volume is profusely illustrated, most of the figures having been taken from previously published papers, with the exception of numerous vegetation maps compiled by the author as introduction to the various biotic areas. Photographs of the vegetation and associated animals are effectively used and add greatly to the text. Although this work brings together much information, there is still room for synthesis and interaction, as the author's epilogue emphasizes:

"While it has on the whole been a pleasure to bring together this information on the bioecology of North America, it seems fitting to say that the basic defects in our fund of knowledge are (1) the lack of studies of both plants and animals in the same community, (2) the lack of quantitative data on the populations of animals and their food habits and on the density of plants, and (3) the lack of consideration of the interrelation of animals and the vegetation. These deficiencies need to be remedied. Let us hope that every effort is made to learn the structure, composition, and dynamics of the original communities of North America before they finally succumb before the advance of civilization."—*William A. Niering.*

Introduction to Integrated Semiconductor Circuits by A. J. KHAMBATA; 233 pages; $7.50; John Wiley & Sons, 1964.

This volume, one of several currently available and reporting on the present status of microelectronics, is essentially a compilation of and guide to some of the recent literature generated by this newest phase of applied electronics. Cast in this role, Khambata's efforts best serve as a preliminary guide to the fabrication processes, design techniques, and currently available hardware for integrated circuits.

Following some general introductory observations, and a review of semiconductor and thin film technologies, the author describes several broad approaches to microminiaturization. Integrated circuits (embodied in semiconductor blocks), the multiple chip approach, thin film circuits, and hybrids of these, are introduced to the reader. Each approach is presented with its unique advantages and disadvantages, fabrication processes, and examples. Most of the illustrations are not academic designs, but rather specific studies of appropriate contemporary

commercial circuits. Much of the material herein presented will be made obsolete by rapid changes in technology.

Finally, the reader is presented with inadequately short chapters on "Testing, Reliability and Maintainability," and predictions of future developments in the state of the art. Appendices review in detail many of the major commercial product lines now available (1963).

Certain specific comments on the text are appropriate. The references given are too few, both as footnotes in the body of the text and as an adjunct bibliography. Particularly in an introductory text, such a finding cannot be neglected. Clearly, it has not been possible for the author to go into depth in any given area, and many worthwhile references are mandatory.

Secondly, very little, even by way of review, is presented concerning the often inherent distributed network properties of microminiature devices. And last, but by no means least, Khambata's own field of digital logic circuits is described and referenced in considerably more detail than other topics, particularly to the exclusion of linear circuits.—*Ralph W. Wyndrum, Jr.*

The Chemistry of Wood, edited by B. L. Browning; 689 pages; $25; John Wiley & Sons, Interscience, 1963.

There has been an increasing need in recent years for a new book on the subject of the chemistry of wood and its components to bring up to date the many advances in this field since the last book of this type was published. This book with its extensive bibliographies at the end of each chapter should serve as an invaluable reference both to the research scientist and to the student in this subject.

By selecting men of recognized authority in their respective fields of morphology, cellulose, hemicelluloses, lignin, and the extractives, to write the different chapters, the latest thoughts and advances are covered in a way probably not possible by a single individual. The chapters on the chemistry of developing wood and on bark are much needed adjuncts to a book of this sort and the chapter on wood as a chemical raw material should serve to stimulate both fundamental and applied research in this fascinating subject.

In a subject in which there are many details that are still largely unknown, there are bound to be differences of opinion with the published statements. This text is on such a theme and no doubt will find among its readers those who differ with the authors on particular details but as a whole the book is to be recommended as a valuable reference to all those working with the chemistry of wood.—*Leonard F. Burkart*

Synthetic Fibers in Papermaking, edited by O. A. Battista; 340 pages; $14; John Wiley & Sons, Interscience, 1964.

This book is the product of twelve outstanding experts in the field of synthetic fibers. Commercial fibers as well as laboratory curiosities are treated with enthusiasm. It is an excellent, well-organized source of technical data. These data are presented in a comparative manner and facilitate the reader's comprehension. Uses of metal, glass, and ceramic fibers are detailed, along with their applications to space age demands. It discusses market potentials in America and in Europe, deals with the opportunities of blending synthetics with natural fibers, attempts to link fiber properties to sheet properties, and elaborates on the technology already developed to handle the new fibers. This book has depth as well as breadth and will satisfy the beginner as well as the expert.

The reader may be left with the impression that synthetic fiber papers are due for a great expansion in the immediate future. This is a highly questionable conclusion in view of the high cost of synthetic fibers. As implied, better marketing efforts as well as more efficient papermaking techniques will be helpful in broadening applications. However, it is doubtful if these are sufficient to carry the use of synthetic fibers beyond very special

papers. Inexpensive natural fibers will continue to satisfy the basic paper needs.

The book, with its enthusiastic presentation, will be a stimulation to many and should be helpful in encouraging more research effort in this challenging field of synthetic fibers in papermaking.—*James J. Eberl*

Photoelectric Astronomy for Amateurs, edited by F. B. Wood; 223 pages; $6.50; The Macmillan Co., 1963.

My first reaction to this excellent book was that it was mistitled. Many, if not most, *professional* astronomers will find it to be a rich mine of information. However, many talented amateur astronomers, particularly variable star observers, could be making far more valuable observations than they are now with the expenditure of a few hundred dollars and perhaps a hundred hours of fascinating shop time. I speak from experience. At the moment I am completing my seventh photometer; three of these were built in my own shop at home with a small circular saw, a drill press, and the usual assortment of hand tools. As a professional astronomer, I very much hope that this book will stimulate similarly equipped amatuers to try their hand at photoelectric astronomy.

There are seven chapters in all. Harlan Smith, Director of the MacDonald Observatory begins the book with a fine review of astronomical photometry and a brief introduction to the details to follow. Chapter 2 by A. D. Code, Director of the Washburn Observatory, nicely describes various designs of photometers and their associated electronics. The next chapter, written by one of the pioneers of photoelectric work, Gerald E. Kron, discusses photocells and filters, while Chapter 4 by Frank Bateson, the well-known New Zealand astronomer, tells how the observations are made in practice. The next two chapters contain valuable lists and information about intrinsic variable stars and about eclipsing systems. The authors are both experts in the area: Helmut Abt of Kitt Peak and Frank Bradshaw Wood of Flower and Cook Observatory. It is Professor Wood who should be congratulated for bringing the volume into being. The last chapter by John Merrill tells how the raw data are reduced, polished, and made available to others.

A book of this sort has been needed for a long while. It is gratifying to see such a splendid piece of work compiled by Professor Wood and his associates.—*William Liller*

The Feynman Lectures On Physics, Vols. I & II, by R. P. Feynman *et al.;* 568 pages; $8.75 per volume; Addison-Wesley Publishing Co., 1963.

This series of lectures, originally given as a two-year introductory course in physics at Caltech, is designed primarily to meet the needs of the brightest and most enthusiastic students. Its special goal is to maintain the interest of these students who, having heard much about the theory of relativity, quantum mechanics, and elementary particles, have still to master the fundamentals of inclined planes, rigid rotators, and accelerating bodies. The resulting volumes, although challenging and exciting to the best students (and to their teachers!), are, in their present form, neither particularly well suited nor completely adequate as texts for an introductory course in physics. The hard core of such a two-year course consists, in the opinion of this reviewer, not of lectures—no matter how brilliant they might be—but rather of examples, applications, and problems. In the original Feynman course, this material was covered primarily in recitation sections, and, for the most part, it has not been integrated into the present volumes.

A particularly intriguing chapter in Volume I entitled "Symmetry in Physical Laws" introduces the students to symmetry operations such as translations in space and translations in time and emphasizes the relationship between these operations and their corresponding

conserved quantities—in this case, linear momentum and energy. Parity is introduced only to be violated. The section on antimatter is followed by a concluding section on broken symmetries. In another chapter with the prosaic title "Ratchet and Pawl," a ratchet and pawl become a novel engine with which to study reversibility and the second law of thermodynamics. While Volume II deals primarily with electricity and magnetism, it is broad enough to include chapters on elasticity and fluid flow and is thorough enough to provide, as is needed, introductions to differential and integral calculus of vector fields and, later, to tensors. The third and final volume, still to appear, will be devoted to quantum mechanics.

With a select group of students and with able teachers such as Feynman, the lectures promise to serve as the basis for a remarkably successful introduction to physics. As a highly original reference, the lectures should also help the less gifted instructor to lift his course out of the stultifying quagmire which so often threatens it. Nevertheless, the lectures probably will not replace the several comprehensive and rather up-to-date texts already available.—*David E. Yount*

Lattice Theories of the Liquid State by J. A. Barker; 133 pages; $8.50; The Macmillan Co., Pergamon 1963.

The liquid state is a great frustration to the theoretician who wants to understand macroscopic properties in terms of the mechanics of the constituent molecules. The use of statistical mechanics to determine even the relatively simple equilibrium properties of liquids has proved difficult and unsatisfying. These difficulties are of course much greater for the transport properties. Even the logical necessity of the solid-liquid or liquid-vapor phase transitions has yet to be demonstrated by convincing statistical mechanical arguments.

This short book presents a thorough and lucid survey of the so-called "lattice" theories of the liquid state at equilibrium, one of the two broad classes of theoretical approaches to the

problem of liquids. As opposed to the "distribution function" theories, lattice theories start from an approximate description of the molecular lattice in the liquid and derive the bulk properties to be expected therefrom. Distribution function theories, on the other hand, emphasize the process by which the liquid structure is determined by the intermolecular forces. Lattice theories may be viewed as an approach to the liquid by way of the solid, while distribution function theories approach the liquid by way of the gas.

The list of chapter headings gives the particular lattice theories described in this book: "Monte Carlo" and molecular dynamics methods, simple lattice theories, the Lennard-Jones and Devonshire theory, variational theory and the cell model, a detailed cell theory, cell cluster theory, hole theory, tunnel theory, the radial distribution function, and quantum effects. The salient features of each theory are given, along with a list of major references and an informal critique. The author is well qualified for the job, since he has contributed to many of the theories himself. No theory of the liquid state may now be said to be satisfactory. This field offers a great opportunity.

Anyone planning to read research articles or do research in this field is well advised to study this book first. Much time and effort would be saved thereby. The general reader with a background in elementary equilibrium statistical mechanics could profitably read this book to increase his physical understanding of the liquid state.—*Frank C. Andrews*

Keys to The Trematodes of Animals and Man by K. I. Skrjabin, *et al.;* English translation edited by H. P. Arai, translated by R. W. Dooley; 351 pages, 919 figs.; $10; University of Illinois Press, 1964.

In 1947 Academician Skrjabin and associates began publishing *The Trematodes of Animals and Man*, a compendium of the world's literature on systematic trematodology. Though printed in Russian and not easily obtained, this set (now 21 volumes, over

14,500 pages) is a major reference. Each taxon is defined, almost every species is figured, some new concepts are introduced, and useful keys are provided.

Designed to be used in conjunction with the Russian set, the 20 chapters of the *Keys* correspond to volumes and keys are in sequence. I selected five volumes for scrutiny. The keys are accurately translated and carefully edited, yet offer only clues—not explanations—to *why* the Russians introduced changes and new taxa. Some typographical errors are perpetuated from the original, though a number were corrected; occasionally errors are introduced. *Adenogaster* Looss, 1901 (fig. 867) is omitted from the Pronocephalinae key (page 83); a note at the end of the same key is incomplete. A note following the *Macravestibulum* key (page 85) is omitted altogether. Whitfield's method of transliteration differs somewhat from the Russian method, and Dooley's strict adherence to Whitfield often results in two spellings for the same name: Issaitschikoff and Isaychikov, Erschoff and Ershov, Orloff and Orlov, etc.

The *Keys* offers a bargain collection of figures of typical species. Arranged six per page, they are reproduced well, though a few, especially schistosomes, suffer from reduction. Figures 245 and 911 are upside down. Detail drawings are rare.

The index, designed for the *Keys*, is useful but incomplete for the original set because the latter has never been indexed. It could be even more useful if species were listed independently rather than under genera, and if figures were included—preferably cross-indexed by genus and species.

Criticism notwithstanding, English-reading workers who have spent laborious hours translating Cyrillic—and those who haven't tried!—will welcome the *Keys*. One must agree with Editor Arai that a complete translation is desirable. In the meantime, the *Keys* serves as an introduction and a valuable "next best thing."—*Mary Hanson Pritchard*

Interpretation of Mass Spectra of Organic Compounds by H. BUDZIKIEWICZ, *et al.;* 271 pages; $8.75; Holden-Day, Inc., 1964.

It is only recently that the potentialities of the mass spectrometer for structure determination have been widely recognized by organic chemists.

The classical method for the identification of an organic molecule consists of two steps. First, the various functional groups present are identified; secondly, the skeleton of the molecule is systematically degraded to simpler units and these are similarly characterized. This approach may take a considerable time, and require a sample of at least a few tenths of a gram. The mass spectrometer, when applied to this problem, carries out almost identical processes, but in an extremely rapid and clean fashion, while the amount of sample needed may be as little as or even less than one microgram. Because of this similarity, the mass spectrometric approach is readily appreciated by organic chemists.

The present book comes from an organic laboratory where extensive use has been made of mass spectrometric techniques. The theme of the book is that fragmentation in the mass spectrometer proceeds by mechanisms and intermediates which are governed by the functional groups present in a very similar way to ordinary organic reactions. The book comprises eleven chapters, each devoted to one functional group or structural feature. Typical chapters cover the carbonyl compounds, the alcohols, the ethers, the amines and amides, the cyclic amines, the cyanides, the alkyl halides, the bicyclic ketones, aromatic hydrocarbons, tropones and aromatic heterocyclics. Each is dealt with at length, and the discussion is conducted throughout in terms already familiar to the organic chemist.

Insufficient mention is perhaps made of the situation where there are several functional groups competing to determine the over-all cracking pattern of a given molecule, but this is not a very serious criticism in the present state of the art. The book does not give any

details of techniques, nor is there any technical information regarding mass spectrometers.

The book is adequately referenced, well-produced, and will be found extremely useful in any laboratory where mass spectrometry is applied to organic analysis. Perhaps more importantly, it is an excellent introduction for the organic chemist who wishes to know how mass spectrometery can be applied in his own work, or who wishes to be able to assess the many publications describing such applications which are now appearing.—*James D. Morrison*

Temporal Organization in Cells. A Dynamic Theory of Cellular Control Processes by B. C. Goodwin; 163 pages; $6.50; Academic Press, London, 1963.

When students of cellular regulatory mechanisms gather, they are likely to write simple differential equations to describe the systems they are studying, in the hope of distinguishing between different models on a kinetic basis. This is usually done casually, and always with an eye on experimental tests of the models. In this book Brian Goodwin has developed a set of equations for some specific regulatory mechanisms in a way that is far from casual, and it will not be easy to test his theory experimentally. His intention is to develop a fundamental physical theory that will serve cell biology as well as mechanics and thermodynamics serve physics and chemistry; it is hard to say how well he succeeds.

Goodwin distinguishes between "weak" and "strong" coupling in cellular control mechanisms. The former involves simple competition, etc.; the latter involves specific regulatory circuits of the well-known Jacob-Moned type. Sets of differential equations are developed in considerable detail for a few of the simplest circuits. In some cases, it can be shown that the concentrations of certain components will vary with a periodic wave-form, in accordance with recent experiments that indicate that biological rhythms may arise directly from the regulatory mechanisms of protein synthesis.

Most of the book is a development of a truly unique approach to the problems of control. Recognizing the limitations of the more deterministic approach, Goodwin presents the beginnings of a statistical theory. On analogy with the well-known thermodynamic functions, he defines "talandic" functions (Greek, talantosis = oscillation) that have some of the characteristics of a temperature, a free energy, and so on. These variables can then be used to characterize the state of the cell. The theory is expanded and applied to some known systems, particularly those that exhibit periodic rhythms.

Goodwins's theory cannot really be evaluated properly yet. First, he has only made a beginning, though a very impressive one. Second, the theory must await many experimental tests—not so much to evaluate its truth as its usefulness. The classical variables of physics, and especially thermodynamics, are useful largely because, once understood, they give us a strong, intuitive feeling for the behavior of certain physical systems. Whether Goodwin's concepts give us this same insight into biological systems is yet to be seen. I do not mean to damn him with faint praise; this is a unique and exciting approach to some important problems, and it may well be a major breakthrough in biological theory. But Goodwin himself is the first to admit its present limitations.—*Burton S. Guttman*

The Making of the Electrical Age, from the Telegraph to Automation by HAROLD I. SHARLIN; 248 pages; $5.95; Abelard-Schuman Limited, 1963.

This book, written "for the scientist and the laymen," tells the story of the development of electrical communications (the telegraph, telephone, and radio) and electrical power (the generator, motor, and central power station). It closes with a few brief pages on the computer and automation.

Both European and American developments are described, and the entrepreneur is given, quite properly, almost as much attention as the scientist

and engineer. We learn that the first practical generator was built to provide power for, curiously enough, electrotyping and electroplating. Later came the carbon arc lamp and the electric motor. The first motors drove electric street cars, and the first of these in the United States ran through the streets of Richmond, Virginia in 1888.

The main fault of this book is that the makers of the electrical age do not live as people. They enter these pages, at their appointed time in history, to connect this particular wire in this particular way in this particular circuit, or to furnish some large sum of money, usually someone else's, to the furtherance of some project with a dubious future, and then depart, like so many faceless robots. In the section on radio, Marconi arises like a wraith out of Italy and is soon dominating the international scene. One wonders what kind of a man he was and what background he came from. The same is true of Cyrus Field, the American financier, who pushed the great enterprise of the first laying of a telegraph cable across the Atlantic to a successful completion.

A less important defect is the omission of any comment on the crucial role played by magnetic materials. Without the flux-multiplying power of iron the electric generator and motor would be nothing but scientific toys. It is indeed a most remarkable fact of history that, when Faraday and Henry and their followers needed this power, they found it right at hand in iron, so cheap, so common, and which later work has shown to be the best element for this purpose in the whole periodic table.—*B. D. Cullity.*

Keoeeit—The Story of the Aurora Borealis by William Petrie; 134 pages; \$5; Macmillan Company, Pergamon, 1963.

This is a delightful story of the aurora borealis which is one of the most spectacular and fascinating wonders of nature. The book starts with an excellent poetic chapter where the reader will find an Eskimo story about the great light of "keoeeit." The author describes the various technical efforts to explore the aurora and other upper atmospheric phenomena, such as all-sky cameras, auroral radars, balloons, rockets, and satellites.

Three chapters are devoted to describing various aspects of the aurora, such as their forms and variations in space and time. The descriptions are accompanied by a large number of photographs and paintings and other illustrative diagrams.

Then, one chapter is devoted to the analysis of "light" from the aurora, or the auroral spectroscopy, in which the author himself is one of the leading authorities. The subsequent chapter is a popular account of the aurora-associated phenomena, such as "hiss," radio wave absorption, and geomagnetic storms.

In the last chapter, the author attempts to cover complicated solar-terrestrial relationships leading to the cause of the aurora and geomagnetic storms. In it, solar radio waves, solar wind, solar cosmic rays, and the Van Allen radiation belts are well described. It is an extremely difficult task for any person to describe interactions between solar gas and the earth's magnetic field in a comprehensive way. In fact, it is still a highly controversial matter among workers in this field.

Doubtless, the book will serve as an excellent and popular introductory book to the readers who are interested in not only the aurora itself, but also some of the various efforts toward space exploration.—*S. I. Akasofu*

The Measure of the Moon by Ralph B. Baldwin; 488 pages; \$13.50; The University of Chicago Press, 1963.

In the author's words this book forms a sequel to, not simply a revision of, his 1949 work *The Face of the Moon.* Baldwin very clearly states in the introduction that a meteoritic origin for the great majority of craters and the large circular maria is accepted as correct and that the book will contain no attempt to prove or disprove this idea.

The book is basically divisible into four sections containing several chapters

each, although the author does not draw attention to this subdivision. The first section discusses meteoritic structures on earth and forms a worthwhile introduction to the moon's surface, as well as a valuable summary of investigations and information on the known and probable terrestrial meteoritic craters and cryptovolcanic structures. The second section is concerned entirely with craters and discusses the characteristics of explosion craters, terrestrial meteoritic craters, and lunar craters; the relationships between apparent diameter, true diameter, rim height, rim width, apparent depth, true depth, depth of explosion, depth of brecciation, and the possible changes in these parameters with age; and the determination of energies needed to produce meteoritic craters. The third section deals with observations on the moon's motion and shape and the determination of the nature of lunar-surface materials by reflected light and the use of heat measurements at infrared and radio frequencies. The fourth section attempts to summarize the ideas of various workers, including those of the author, on the origin and development of a variety of pertinent lunar features. A separate chapter is devoted to each of the topics which include tektites; theories of moon history; maria; lava flows; lunar atmosphere; rays; craters with central peaks; rilles, wrinkles, and faults; lunar grid system; domes; heat balance; magnetic field; and recent changes on the moon. An initial chapter outlining the problems of lunar history and a final summation chapter complete the text of the book. Appendices include derivations of the relationship between the distance of the moon and geologic time, and the lunar tidal bulge as a function of the moon's distance. A set of tables forms the bulk of the appendix and gives detailed data of craters on both the earth and moon, as well as those produced by chemical and nuclear explosions.

Baldwin brings to this book a long-continuing personal background of experience in lunar problems, and the detailed and abundant references to the literature reflect this background. Un-

fortunately, these references and summaries of past work are often given in a rapid sequence with but little summary or aids along the way to assist the reader in assimilating the information, or in obtaining a comprehensive view of the significance of the studies described. The chronological listing of investigations occasionally makes it difficult to follow Baldwin's line of reasoning. Presenting older or incorrect data first, before the significance of the measurements are made known, is confusing to the new reader.

Common throughout the book are anthropomorphic or folksy expressions. Occasionally, these expressions are interesting, but usually they suggest that the author is uncertain of the real nature or cause of what he is describing. Several astronomical terms or abbreviations are used in the text without definition.—*Carl Bowin*

Water Metabolism in Plants by T. T. KOZLOWSKI; 227 pages; $3.95 paper; Harper & Row 1964.

This volume is another contribution to the paperback series of biological monographs being published under the editorship of Professor Allan H. Brown. To a degree its title belies its contents, since as much emphasis appears to be placed on physical features of plant water relations as on metabolic aspects. Even so, the book performs the useful function of providing an up-to-date presentation of much of the recent literature on plant water relations.

The main subject matter divisions include sections on xerophytism and the plant water balance, water relations of plant cells and tissues, absorption of water, water transport, loss of water, and the effects of water deficits on plants. Most sections are, unfortunately, treated rather superficially, due in part to the wide range of subject matter covered. However, the author has presented more detailed discussions of conflicting viewpoints concerned with such phenomena as nonosmotic water movement, ascent of sap, and the role of transpiration in ion uptake, and these discussions add substantially to the book's value. One shortcoming, which it is thought will be felt by many readers, is the absence of expression of the author's own views on these various controversial issues. One has the feeling that in an effort to be impartial the author has sometimes brought out various viewpoints without indicating to the reader the one which he considers most applicable.

It is thought that the book will find its largest audience among those plant and soil scientists not strongly trained in plant physiology who wish to obtain background information on plant-water relations. Among this audience will probably be students in forestry, horticulture, and related areas who have already completed a general botany course and wish to extend their knowledge into this more specialized area.—*R. O. Slatyer*

The Rhinencephalon & Related Structures: Progress in Brain Research, Vol. III, edited by W. BARGMANN & J. P. SCHADÉ; 252 pages; $12.75; American Elsevier Publishing Co., 1963.

This volume contains a series of lectures delivered during a symposium on the rhinencephalon which was held at the Third International Meeting of Neurobiologists, during September 26–29, 1962.

There are fourteen lectures on various topics, all of which are in English except two, and these have English summaries. There is also another chapter which is a general discussion of the confusion created by the term "rhinencephalon."

Three of the lectures deal with the amygdala and three with the hippocampus. Most of the others deal with the more strictly olfactory parts of the brain. The emphasis throughout the volume is on anatomy.

One chapter presents the anatomical evidence for three major projection fields of the amygdala, including the more recent evidence for amygdaloid connections with the dorso-medial nucleus of the thalamus. Also discussed are microelectrode recording studies of

amygdaloid cells and evidence for connections between the amygdala and the central sympathetic areas.

The lectures on the hippocampus include a proposed common embryological origin for the hippocampus and the amygdala, a quantitative histological analysis of some parameters in uncinate area of the human hippocampus, and an electron microscope study of hippocampal cells.

There are also lectures on the capillary density in the olfactory lobes in the brain, the gross anatomy of the rhinencephalon and related areas in ungulates, and afferent and efferent connections of the rhinencephalon.—*W. R. Klemm*

Eugenics: Hereditarian Attitudes in American Thought by Mark H. Haller; 264 pages; $6; Rutgers University Press, 1963.

This comprehensive account of the origin and often sharply conflicting currents of the eugenics movement, chiefly in Britain and America, is scholarly, informative, and highly readable. Some geneticists, aware of the misunderstanding of the human implications of their subject prevalent among many specialists in social sciences and humanities, will be surprised that Haller, a historian, should have a grasp of the field so penetrating, well rounded, and—from the standpoint of modern genetic knowledge—sound and fair.

It is instructive to read how the high hopes of the early eugenists, many of whom were social radicals, such as Shaw, Sidney Webb, Havelock Ellis, Noyes, David Starr Jordan, and J. McKeen Cattell, foundered as the idea of human genetic betterment was increasingly taken over by social reactionaries. These wishful thinkers and egotists not only over-simplified heredity and minimized environmental effects, they identified eugenics with the increased multiplication of their own ethnic or social group, and with the reproductive suppression of groups having less prestige. Their "eugenics," by paving the way for the atrocities of the Nazis, suffered the same deserved fate as Nazism in general, but it also dragged with it into obloquy more reasonable views of eugenics. Yet, as Haller shows, even in the early days of the movement, the seeds for the later perversions had been sown in Galton's and some other eugenists' gross underestimation of the force and subtlety of cultural influences.

In concluding, Haller deals with the more balanced view of human progress, embracing *both* genetic and social advances, that is at last gaining ground among genetically educated persons. In my opinion, Haller at this point somewhat over-rates the possibilities of deliberately altering the DNA, but underrates those latent in presently attainable techniques of germinal choice. However, an adequate discussion of this matter would require another book.—*Hermann J. Muller*

Generic Names of Orchids by Richard E. Schultes & Arthur S. Pease; 331 pages; $12; Academic Press, 1964.

This is a very welcome work for it fills a long existing need. The family *Orchidaceae* is, no doubt, the largest, most diverse, and widespread of all plant families. The number of its genera has been estimated as being up to 800 and the species may number up to 35,000. The names of orchids are no less diverse than the plants themselves. They range from the whimsical *Aa* to the highly descriptive *Zygostates*. In most instances the origins and meanings of these names are difficult if not impossible to trace. The existence of bigeneric hybrids with names which are also often "hybrids" only adds further complications.

In this book Schultes and Pease have traced the origins and meaning of orchid names and outlined the information in an admirable manner. Greek and Latin origins of various names are always presented in their original forms and accompanied by English translations. In the case of names which commemorate a person, a thumbnail sketch of the person in question is always given.

The quality of this work is further enhanced by good, concise chapters on the rules and nature of botanical nomen-

clature, orchid morphology, and orchids and economic botany. A listing of all subfamilies, tribes, and subtribes of the *Orchidaceae* and a key to their geographic distribution are also given and each genus in the text is properly identified as to its geography and taxonomy. Numerous excellent drawings of orchids illustrating their morphological peculiarities and photographs of well known orchidists of the past are also included. The book therefore offers a wealth of information in a concise and easy-to-use form.

A list of names of bigeneric and multigeneric hybrids concludes the book. The origin of most of these names is self evident, since, like the plants themselves they are "hybrids" of existing names. In a few instances, however, bi- or multi-generic hybrids have names of different origins. Unfortunately, these are not given in detail. The authors have chosen not to indicate which generic names are considered valid at the present. This seems to be unfortunate, since such a listing may have made this book into the much needed standard reference work on the nomenclature of orchid genera.

As a whole *Generic Names of Orchids* can be highly recommended as an excellent work which would no doubt be of considerable value not only to orchidists but to all who have an interest in the derivations of plant names.—*Joseph Arditti*

Quantum Theory of Solids by C. Kittel; 435 pages; $13.50; John Wiley & Sons, 1963.

Kittel has valiantly attempted an essentially impossible task: to treat in one volume the quantum theory of solids in an advanced and detailed fashion, yet in a form suitable as a graduate text for "theoretical physicists generally and those experimental solid state physicists who have had a one year course in quantum mechanics." The result is a book so concentrated that the average page contains as many lines of equations as of text. The reviewer does not believe that a reader can follow any chapter unless he is already familiar with most of

the material in it. The Mathematical Introduction, for instance, could be understood only by one knowing thoroughly the time-dependent perturbation theory in the form now being used in field theory. Yet the student who wishes to read enough other literature to gain the necessary background will be continually frustrated by the shortage of references, a policy which the author specifically defends.

The broad and stimulating coverage is indicated by chapter headings: Acoustic Phonons; Plasmons, Optical Phonons, and Polarization Waves; Magnons; Fermion Fields and the Hartree-Fock Approximation; Many-Body Techniques and the Electron Gas; Polarons and the Electron-Phonon Interaction; Superconductivity; Bloch Functions; Brillouin Zones and Crystal Symmetry; Dynamics of Electrons in a Magnetic Field; de Haas-van Alphen Effect and Cyclotron Resonance; Magnetoresistance; Calculation of Energy Bands and Fermi Surfaces; Semiconductor Crystals: Energy Bands, Cyclotron Resonance, and Impurity States, Optical Absorption and Excitons; Electrodynamics of Metals; Acoustic Attenuation in Metals; Theory of Alloys; Correlation Functions and Neutron Diffraction by Crystals; Recoilless Emission; Green's Functions—Application to Solid State Physics.

In spite of its unsuitability as a text for students on the level for which it is intended, this is a monumental work which will be of great value to workers of greater advancement and sophistication.—*J. C. Slater*

Selected Values of Thermodynamic Properties of Metals & Alloys by R. HULTGREN, *et al.;* 963 pages; $12.50; John Wiley & Sons, 1963.

This collection of metal and alloy information must take its place beside Hansen's *Constitution of Binary Alloys* and Pearson's *Handbook of Lattice Spacings* as being among the most important reference works available. A measure of its importance is given by the fact that the major support was provided by the U.S. Atomic Energy Commission with added grants coming from ten industrial organizations.

The aim of the authors was to evaluate and present the most reliable thermodynamic data available for elements (67) and binary alloy systems (180). This is no easy matter because of the great variety of methods and the difficulty in obtaining accuracy; the authors have found vapor pressure measurements which disagree by a factor of a hundred or more! "Evaluations of the validity of experimental data have been accomplished mainly by testing for self-consistency, consistency with known thermodynamic relationships, consistency with the phase diagram, and agreement with other measurements." It is a difficult matter for the scientist or engineer to assess the data reliability on his own. Commensurate with this difficulty is the importance of good thermodynamic data to science and technology. This book, under way since 1955, is a carefully drawn survey. There are comprehensive data for the elements: electronic specific heat, heat capacity over a wide temperature range, vapor pressure, enthalpy, entropy, free energy, and references. For the binary systems, the data are usually less extensive but there are also partial thermodynamic quantities and data on composition dependence. Each element or alloy system is introduced with a brief discussion and the data are provided in the form of tables of standard thermodynamic quantities.

The authors have succeeded in creating a useful, clearly written, and intelligently presented reference work. The relatively low price is presumably due to the subsidization of the project and to the off-set printing, the latter not detracting from the end product.—*Leonard Muldawer*

Electromagnetic Theory for Engineering Applications by W. L. WEEKS; 744 pages; $18; John Wiley & Sons, 1964.

It is a pleasure to recommend this book. In the first place, it fills a definite need. In recent years, we have had two outstanding graduate texts in electromagnetic theory (by Danofsky and

Phillips and by Jackson) but these have been aimed at students interested in the behavior of fundamental particles, and are heavy in topics such as special relativity and radiation from accelerating charges. The present book concentrates on transmission lines, waveguides, and antennas, living up to the promise implicit in the title. Secondly, this book is well-written and nicely organized, so that it can be used for courses of various length or for self-study. The style is, in fact, rather informal and might tend to give the reader the feeling that the material is not as difficult as it actually is. For once, the blurb on the dust-jacket accurately describes one of the outstanding merits of the book: the introduction to the use of the Green's function, in terms of a superposition of delta functions, is the clearest that has yet appeared in any text.

The author assumes at the beginning that the student is familiar with Maxwell's equations and with mathematics at the advanced calculus level. Since these topics are normally covered by the end of the junior year in most electrical engineering schools, this book would even be suitable for a final year text in antenna and wave-guide theory. The first chapter reviews the prerequisite material, discussing the operational definitions of the field quantities and their representation by complex quantities. This survey should also help the student obtain a more unified picture of the topics which tend to get jammed into the introductory engineering courses. The second chapter discusses the transmission line equations, and compares the three types of solutions: conventional, Green's function, and integral transform. Chapter III covers rectangular wave guides in great detail, and some related problems. Chapter IV introduces Huygens' Principle and this is applied to a discussion of complementarity in later chapters. The coverage of problems in spherical and cylindrical coordinates is quite extensive, and partly accounts for the length of the book. An interesting application, which so far has been covered only in the literature, is an elementary description of the log periodic antenna.

This book may also be regarded as an introduction to "Field Theory of Guided Waves," by R. E. Collin, with which it heavily overlaps. However, Weeks' book is written with the problems of the student given every consideration; yet it is not simply a primer of classical ideas, since there is ample contact with the original literature and a number of references to problems which are either difficult or impossible to solve.—*Allen Nussbaum*

Ancient & Medieval Science: From the Beginnings to 1450, edited by RENÉ TATON, translated by A. J. POMERANS; 552 pages; $17.50; Basic Books Inc., 1963. Vol. 1, *A History of Science*.

This survey of the early history of science is the first of a four-volume series to be translated into English. It is undoubtedly the best general history of science in any language. The present volume, the work of twenty-one specialists, begins with the Egyptians and their "cook-book" approach to science (collections of recipes to be memorized rather than principles), and moves on to Mesopotamia, where arithmetic and astronomy were developed more systematically. Both cultures were prescientific in the sense that scientific information was considered merely a practical tool. True explanation was the domain of religious mythology. The treatment of Greek, Alexandrian, and Roman science concentrates on mathematics, physics, astronomy, and medicine. Compared with Sarton's final two-volume survey of the same period, the present treatment is briefer, more objective, especially in the treatment of Plato, and less given to biographical and anecdotal details. However, an unfortunate interchange of "deductive" and "inductive" confuses the evaluation of Plato and Aristotle (p. 229).

Since the present work aspires to completeness, every culture which made any contribution to science is considered: ancient India and China; pre-Columbian America; medieval developments among the Arabs, Indians, and Europeans. The emphasis is invariably on an accurate presentation of the factual achievements of science while critical reflection is curtailed. Philosophy and religion are considered only inasmuch as they contributed to the growth of scientific thought, but side issues such as ancient philosophical reflection on the nature of scientific thought or the noetic influence of particular cultural factors are generally omitted. The translation reads well and seems quite accurate, except that the name, "Dijksterhuis" is repeatedly spelled with a "K" rather than a "D" (pp. 370, 532) in the brief bibliographical appendices to the various sections.—*Edward M. MacKinnon*

The Nature of the Natural Sciences by LEONARD K. NASH; 406 pages; $7.50; Little, Brown & Co., 1963.

There is a spectrum of writings in philosophy of science ranging from formal constructions (frequently by mathematical logicians) to general remarks on what actually goes on in science (usually by practising scientists). This book is a good example of the latter. Contributions of this sort have, I think, real value, particularly for those professional philosophers least in contact with science.

Moreover, these contributions may provide raw-material for work in heuristics, that most important subject in the philosophy of science, cautiously ignored by the more formal writers in recent times.

Not that the author of the present book has much sympathy with any attempt at arriving at an explicit heuristic method. Professor Nash shares the contemporary suspicion of methodological straight-jackets, and is rightly impressed by the diversity of lines along which science progresses.

There is a good discussion of "old age" in the life cycle of a scientific theory, and a welcome emphasis on the importance of the correspondence principle in the transition to a new theory.

I think it would be desirable to distinguish more clearly between the use of the term "model" in the sense of interpretation ("dictionary") by way of an (operational) anchoring of the theory

in experiment, and its use in the sense of mechanism, postulated for the sake of explanation, going beyond the strict requirements of the theory.

Minor flaws in illustrative examples include an implied misconception of the mass-energy relationship (p. 185), and the ill-advised application of the term "ad-hoc" to Mendeleev's postulation of elements to fill the gaps in his scheme.

None of the problems in philosophy of science mentioned in this book are discussed in great depth. But there is an attempt at presenting science in the round, including its sociological aspect.

The greatest value of the book lies in the abundance of apt quotations from scientists and philosophers of science.—*H. A. Post*

The Future of the Research Library by VERNER W. CLAPP; 114 pages; $4.50; University of Illinois Press, 1964.

Mr. Clapp's book is an extended explanation of the purpose, work, and goals of the Council on Library Resources of which he is the president. He says (quoting from the foreword), "it tries to identify the problems which now present obstacles to efficient library service and to find methods for overcoming these impediments through new procedures and the application of technological developments."

The huge and increasing volume of information each research library must have or be able to make available for its users can never be solved completely. However, ready access to as much material as possible, local and national (even international) sharing of materials with possible allocation of responsibilities of acquisition and clarification in specific disciplines, and continuing drive of research and experimentation of small as well as large specialized libraries is the way toward meeting the challenge of the future research libraries."

Mr. Clapp sums up a library philosophy which is easy for busy librarians to forget as they become obsessed with

nonessential details or irritations of one's day . . . "The research use of library materials can and must play an increasingly important role for the conduct of human affairs, it follows that the research library of the future must increasingly find ways to promote and facilitate such use. This will not be accomplished by inventing short cuts in cataloging or book arrangement, or by discouraging interlibrary lending, or by short changing users because they do not appear to qualify in terms of scholarly achievement."

Instead, all of us should keep in mind this challenge: "How do we put the research library of the future to greater use."

Mr. Clapp's presentation of the problem, together with an elaborate appendix, is a work to stimulate the ambition and imagination of librarians in the field of research.—*Mary M. Johnson*

Equations of Mathematical Physics by A. N. TIKHONOV & A. A. SAMARSKII; 765 pages; $17.50; The Macmillan Co., Pergamon, 1964.

The contents of this book are a necessary part of the mathematical equipment of the physicist and engineer. The subject is restricted largely to partial differential equations of the second order, with brief mention of higher order equations arising in elasticity. The book was written as a textbook, and is therefore, more expository and requires less mathematical sophistication than Courant-Hilbert (Volume II), which covers much of the same material. The treatment here is classical, with an initial chapter on the classification of second order equations, followed by three chapters on hyperbolic, parabolic and elliptic equations, illustrated by detailed treatments of the wave, heat and Laplace equations. The remaining three chapters are devoted to wave propagation and heat conduction in three dimensions and to some further aspects of elliptic equations. A supplement on special functions contains discussion of Bessel functions, Legendre polynomials, and Chebyshev polynomials.

Each chapter is concluded with a series of problems and several appendices illustrating application of the methods to problems beyond the scope of the Laplace, wave, and heat equations. Typical subjects include vibrations of rods, dynamics of the absorption of gases, temperature waves, Einstein-Kolmogorov equation, equations of the electromagnetic field, diffusion of a cloud, and skin effect. This, in the reviewer's opinion, is one of the more attractive features of the book.

The content is thus more than satisfactory for a course in the partial differential equations of mathematical physics. The style, however, is somewhat dry, with very little attempt being made to provide motivation. Because such a great amount of detail is included in the text, it would appear to be most useful as a reference for the standard methods of solution of linear partial differential equations.—*David A. Conrad*

Handbook of Mathematical Functions, edited by MILTON ABRAMOWITZ & IRENE A. STEGUN, National Bureau of Standards Applied Mathematics Series 55; 1046 pages; $6.50; U.S. Government Printing Office, 1964.

This volume represents the results of a ten-year cooperative effort having the general goal of producing a modernized version of the classic Jahnke-Emde tables of mathematical functions. There are twenty sections, beginning with one listing mathematical constants to as many as 25 significant figures, and going on through sections covering the wide range of such things as elementary functions, elliptic functions and integrals, Mathieu functions, orthogonal polynomials, combinatorial analysis, and Laplace transforms. Extensive material is included on the properties, use, interpolation, and extrapolation of the various tabulated functions, with graphs provided where these are helpful. The tables have been set up so that simple linear interpolation will yield four- or five-figure accuracy, although much higher accuracy can be obtained with more elaborate methods of interpolation. A user might find the physical size, 8 by

10 by 2 inches, somewhat large for easy reference, but there should be no argument about the price of $6.50 representing a real bargain. Anyone who has occasion to do numerical work with mathematical functions should be aware of this volume.—*W. J. Cunningham*

Van Nostrand International Encyclopedia of Chemical Science, edited by A. F. Clifford *et al.;* 1331 pages; $32.50; D. Van Nostrand Co., Inc., 1964.

An encyclopedia can be judged using three criteria: the range of the subjects, the depth of treatment, and, more subjectively, the quality of presentation. This encyclopedia has thousands of articles on physics, chemical physics, physical, organic, inorganic, analytical, industrial, and biological chemistry varying in length from a few lines to 38 pages (Organic Chemistry Nomenclature, a reprinting of the IUPAC rules). The range of coverage is roughly comparable to similar encyclopedias. It is not possible for the depth of treatment of most subjects to be as great in a one-volume work, such as this, as in multivolumed encyclopedias, due to space limitations; for this reason, superficial and incomplete treatment of various subjects is more frequent.

The quality of the articles varies. Physical subjects are presented mathematically where possible; these articles, though terse, seem fairly complete and up-to-date. Organic and inorganic chemical articles are largely descriptive and mainly devoted to defining nomenclature and structures, rather than complete descriptions of reactions and their underlying mechanisms. Articles on analytical chemistry are numerous; recipes for a large number of reagents and their use, particularly in qualitative analysis, are presented. The biochemical articles are out-of-date; they represent biochemical knowledge as of 1940, not 1964. Some factual errors were noted in surveying the encyclopedia.

Instead of putting cross-references at the end of articles, these are printed, in boldface, directly in the text; a very convenient device. Four glossaries with

translations of selected terms from German, Spanish, French, and Russian into English are included; their utility in such a work seems questionable. The wide coverage of chemistry in a single volume, despite the faults which occur, make this encyclopedia a valuable addition to reference libraries.—*David B. Straus*

Chemistry in Premixed Flames by C. P. Fenimore (*The International Encyclopedia of Physical Chemistry & Chemical Physics* Topic 19, Volume 5); 119 pages; $5; The Macmillan Co., Pergamon, 1964.

This book provides a much needed clear exposition on the reaction kinetics and chemistry which can be deduced from studies of premixed flames. In particular, it complements the books by Gaydon and Wolfhard and by Lewis and von Elbe, both of which discuss combustion and flames in more general terms and do not consider specific reactions and rates to any great extent. Fenimore rightly points out that the "activation energy" obtained by applying the Zeldovich or other theoretical burning velocity-temperature equation to measurements of burning velocity may have practical utility but no fundamental significance should be attached to the numerical values. It is necessary to obtain temperature and composition profiles through the flame to deduce any significant kinetics or chemistry.

There are short but adequate chapters reviewing carbon formation, flame inhibition, and ionization and electronic excitation in hydrocarbon flames. While the book is mainly concerned with hydrogen and hydrocarbon premixed flames, other flames such as the decomposition flames of hydrazine, hydroxylamine, nitrate esters, nitrous oxide, hydrogen peroxide, ethylene oxide, and ozone, and flames involving nitric oxide are included.

The author is to be commended for his clarity and for the many references, 283 in number, which include papers from the Ninth Symposium on Combustion and from journals published in 1963.

Indeed, it is essential in a work of this type that the references be up-to-date since this book will probably need revision in half a decade as the study of composition and temperature profiles in flames becomes more widespread. It is, however, thoroughly recommended to all workers and newcomers in this field and also to nonspecialists interested in reading about premixed flames since the theoretical aspects have been kept to a minimum.—*Graham S. Pearson*

Combustion & Propulsion, Fifth AGARD Colloquium: *High Temperature Phenomena*, edited by R. P. Hagerty, *et al.;* 698 pages; $20; The Macmillan Co., Pergamon, 1963.

This particular AGARD volume is one of the best of the series which has been published. In evaluating volumes which are collections of papers, the true worth can be established only by the individual manuscripts. It is regrettable that all papers presented at the colloquium were included. Perhaps, if they were not, the price of the volume would have been lower and the better papers would have become available more readily.

The worth of this book lies in some exceptionally good review articles and a few first-rate original papers. The review articles worthy of note are Hockstein's on the thermodynamic properties of air, Gordon's on the pyrolysis of hydrocarbons, and Blatz and Anderson's on fabrication of plastics. The original papers which stand out are Fay's on plasma boundary layers and Marble's on the dynamics of a gas containing solid particles. Marble's article undoubtedly will become a classic in its field and will be well-referenced hereafter.

The volume is divided into six parts: I. Equilibrium Properties of High Temperature Gases, II. High-Temperature Fluids, III. Physical Processes in Combustion and Propulsion, IV. Transport Properties of Ionized Gases, V. High-Temperature Materials for Solid Propellant Grains in Liners, and VI. Basic Combustion of Rocket Engines Using High Energy Fuels. Most of the

papers in Part VII are the subject of combustion instability in rocket engines and report some new experimental results, but no new definitive contributions can be extracted from this.

All papers are followed by discussions, many of which are critical and worthy of note. In particular, Oppenheim's comments on Glamsdorff's paper "Equations de l'Hydrodynamique des Gas Ionisés" and Crocco's remarks on Barrere and Corbeau's paper "Les Instabiltiés de Combustion dans les Fusies à Propergol Liquids" deserve attention.—*Irvin Glassman*

Vapour Pressure of the Elements by AN. N. NESMEYANOV, translated & edited by J. I. CARASSO; 469 pages; $14.50; Academic Press, 1963.

Vapor pressure data for the elements are used in many areas of science and technology. There are several compilations of vapor pressure data available in the literature, but none are nearly as extensive or as critical as in this book.

Nesmeyanov has surveyed the literature from 1874 to 1961 and lists 695 references. Data are presented for all of the elements except H_2, N_2, O_2, He, Ne, Ar, Kr, Xe, and Rn. For a given element, the results of various workers are presented in separate tables and compared on a single graph on which is plotted log pressure (torr) as a function of the reciprocal of the absolute temperature. The author carefully analyzes the often conflicting results and gives his opinion as to which are the most reliable.

While there are no author or subject indexes, access to the information presented in the book is easily accomplished through the use of the tables which appear at the end. One table lists the elements and the references which pertain to each. The coefficients of the equation relating vapor pressure to absolute temperature were computed from the most reliable data and are tabulated. Molecular composition of vapor phase, melting point, boiling point, heat of fusion, and heats of sublimation are also listed for pressures at decade intervals (10^{-10} torr to 1 atmosphere). A final series of tables gives the

vapor pressure for various temperatures. All of these data are easily used.

The first 117 pages describe in some detail the methods used for determining vapor pressures and assess their sensitivities.

This extensive, critical survey of the vapor pressures of the elements is a well organized, useful book.—*Richard W. Roberts*

Current Topics in Organic Chemistry, Vol. 1 by L. F. Fieser & M. Fieser; 122 pages; $2.75 paper; Reinhold Publishing Corp., 1964.

This inexpensive publication represents a commendable job by the authors, sorting out the enormous amount of literature associated with organic chemistry, and it offers a treatment of important current topics. It follows the pattern of Part 2 of a previous Fieser book, *Topics in Organic Chemistry*, and includes subject-matter selected from journals received in the period from January 1, 1963 to December 15, 1963. There are over one hundred-eighty concise sections each checked and corrected by the authors of the papers cited.

The book will be welcomed by students, teachers, and research workers with particular interest in usefulness and properties of reagents, new reactions, novel and improved syntheses, isolation of natural products, and elucidation of structures.—*E. J. Poziomek*

Nuclear Chemistry & Its Applications by M. Haïssinsky, translated from the French by D. G. Tuck; 834 pages; $22.50; Addison-Wesley Publishing Co., 1964.

This book presents nuclear chemistry in its broadest form; however, a better title would have been "Nuclear Science and Its Applications." Nuclear transformations and ionizing radiation are now used in so many scientific and engineering disciplines that this area of knowledge is a science in its own right, and this book shows how nuclear techniques can be used in such diverse areas of knowledge as geochemistry, astro-

physics, biology, chemistry, physics, and the process industries.

The scientific community owes a debt to Dr. Haïssinsky for his great skill in writing this book. Basic information on such topics as fundamental nuclear particles, nuclear forces and models, and the transuranium elements is combined with applications including reactors, separation of isotopes, tracer techniques, and technological uses. It is a pleasure to see a book written by a real master of an area of knowledge. Too many current publications in nuclear science consist of surveys by various specialists or the proceedings of various symposia collected under one cover. These books generally are of uneven quality and usefulness. Dr. Haïssinsky's book, on the other hand, shows the hallmark of a real scholar—a clear, orderly, and understandable presentation. The same praise applies to Dr. Tuck's work in translating this work from the French.

This book will find wide use among radiochemists, radiation chemists, and other people concerned with the applicaton of radioisotopes and ionizing radiation, and in fact, it is hard to see how they can get along without this book. The price is reasonable for a volume of this size, and the more than 2700 references are a bargain. The reviewer has recommended that his students purchase the book.—*John A. Wethington, Jr.*

Ultracentrifugal Analysis in Theory & Experiment, edited by J. W. Williams; 282 pages; $10; Academic Press, 1963.

"Ultracentrifugal Analysis" contains the papers presented at a "Conference on the Ultracentrifuge" sponsored by the National Academy of Sciences with the financial support of the National Science Foundation. The meeting was held at The Rockefeller Institute from June 18 to June 21, 1962. Editorial comments on certain papers are included. This volume is an important collection of concise reports on the current theory and use of the ultracentrifuge by contributors who are recognized authorities in the field.

The seventeen papers comprising the book are divided into three parts, namely, "Transport Theory," "Equilibrium Theory," and "Practice." An indication of the range of topics covered is given by taking one sample paper from each part; the application of the theory of nonequilibrium thermodynamics to diffusion and sedimentation by G. J. Hooyman in part one, a treatment of sedimentation equilibrium in reacting systems by E. T. Adams, Jr. and H. Fujita in part two, and in the last section, the use of separation cells by D. A. Yphantis.

There are many workers who use the ultracentrifuge routinely for whom the classical equations of Svedberg give sufficient and useful information relative to their particular problem. The highly specialized treatments of, for instance, transport processes will be of limited service in these instances. Nevertheless, the book should not be totally ignored by these researchers. A few articles are of general interest and deserve consideration. For example, the ideas put forth in "Computational Methods of Ultracentrifugation" (R. Trautman), such as supplementing the relative centrifugal force method of reporting preparative procedures by giving the minimum sedimentation coefficient, should have wide application.

On the other hand, investigators who rely mainly on the ultracentrifuge as the principal tool in their research, as well as theoreticians interested in transport phenomena will find "Ultracentrifugal Analysis" of immense value as a record of the current trends in the field and for the new experiments and lines of research suggested. Possible applications of density-gradient centrifugation are discussed by M. Meselson and G. M. Nazarian. The procedure of G. A. Gilbert and R. C. Ll. Jenkins for the study of reversibly interacting systems, whereby a value of the equilibrium constant, k, and model schlieren patterns can be calculated, will probably be employed for an increasing number of reactions. The optical systems for sedimentation analysis described by H. K. Schachman suggest many experiments.

Rather than publish the actual dis-

cussions, the editor chose to indicate briefly the comments of the participants.—*Leon M. Krausz*

Thermodynamics of Clouds by L. Dufour & R. Defay; International Geophysics Series, Volume 6; 255 pages; $10; Academic Press, 1963.

The authors of this valuable book are, in the positive sense of the word, perfectionists. Engaged in the study of the physics of clouds, it became apparent to them that there is a need for a coherent treatment of a large group of problems directly or indirectly related to the thermodynamics of capillarity. Since cloud physics is a branch of modern meteorology in which speculations and expectations are not always kept in the right proportion to solid theoretical, physical analysis, this work really fills a gap and, for that matter, in a very thorough form. In the first part of the book, the general formulas for interface-thermodynamics are developed. The second part describes their application to the phenomena of droplets and ice crystals suspended in the atmosphere. The final section treats the theory of condensation- and freezing-nuclei. In several chapters the authors have included the results of their own recent research. For example: by comparing the nucleation rates of water and ice in humid air they conclude that "even below $-65\,^\circ$C., homogeneous nucleation of water takes place rather than of ice, contrary to the opinion often held since . . . 1940." The bibliography contains many articles from continental journals not all of which may be known to the specialists in this country. Only thus one realizes that the present text is a translation (prepared by M. Smyth and A. Beer) of the original elaboration made in French, at Brussels.—*W. Schwerdtfeger*

Molecular Rearrangements, Part 2, edited by Paul de Mayo; 535 pages; $20; John Wiley & Sons, Interscience, 1964.

The high standards established in

Part 1 of *Molecular Rearrangements* are continued in the second volume, which is concerned with the rearrangements of carbohydrate chemistry (R. U. Lemieux), terpenoids (J. F. King and P. de Mayo), alkaloids (E. W. Warnhoff), amino-acids and peptides (L. A. Cohen and B. Witkop) and steroids (N. L. Wendler). One might expect that a volume devoted to rearrangements of natural products could degenerate into an old-fashioned descriptive treatise, but such is not the case for Part 2. Lemieux, for example, begins his chapter with a discussion of the conformational aspects of "Rearrangements and Isomerizations in Carbohydrate Chemistry." The other contributors likewise emphasize the mechanistic importance of the rearrangements they discuss. Somewhat in contrast to the first volume, there is a surprising homogeneity of style and content. From the copious references, the literature appears to be well covered both from a historical and a modern view. Most of the authors of *Molecular Rearrangements, Part 2* have made scientific contributions to fields discussed in their chapters.

It is difficult to see how this treatise could be improved upon, particularly since it has the considerable advantage over Part 1 of containing the index! The reviewer recommends it for all graduate students majoring in organic chemistry—and for their professors as well.—*Clair J. Collins*

Biogenesis of Natural Compounds, edited by P. Bernfeld; 930 pages; $28; The Macmillan Co., Pergamon, 1963.

In this book an attempt is made to summarize present knowledge of the pathways by which the major natural products are synthesized. The book contains eighteen chapters, each dealing with a major group of natural compounds. Some of the pathways have been reviewed often (for example, those of amino acids, purines, and pyrimidines), and sometimes far less frequently (as with the phenolic plant products, terpenes, and alkaloids). The value of reviewing some of the better known pathways can be questioned, since

adequate compilations are already available. However, the book does seem to do a real service in reviewing the less-often discussed pathways and in bringing them together into a convenient reference work.

As with any compilation of work by a large number of authors, the quality and interest of the chapters vary. This reviewer found several of the chapters interestingly and well written, whereas others were quite ordinary reviews.

Unfortunately, with volumes of this type the reviews are often behind the published work by an uncomfortable margin. The literature is summarized to approximately 1960, which might be considered as reasonable for this complex volume.

Repetition is almost unavoidable in a work of this sort. The same basic schemes apply to a variety of pathways. Hence, they are dealt with by several authors, unless editing is especially critical. This seems to be a difficulty with this work, as with others.

In spite of the above shortcomings, this reviewer found the book useful as a reference work and in teaching. It should provide a welcome source book for most libraries. Whether the individual research worker will wish to purchase it will depend on his specialized interests and on the funds available.—*Arthur B. Pardee*

Introduction to Satellite Geodesy by IVAN I. MUELLER; 415 pages; $15; Frederick Ungar Publishing Co., 1964.

Historically, geodesy has been concerned with the determination of the size and shape of the earth, the location of points on the earth's surface, and the description of the gravity field on or near the surface. Satellites provide a particularly powerful method for investigating the earth's gravitational field and, to a lesser extent, for determining the relative locations of points on the earth's surface.

Mueller introduces the subject of satellite geodesy by considering the use of solar eclipses and occultation in geodesy. This discussion takes up one-

third of the book and provides the connection between the classical methods of geodesy and the newer method utilizing satellites. Mueller treats orbit determinations; the observation of artificial satellites; the reduction of observations, including visual, photographic, and electronic; and the interpretation of these observations in terms of the earth's gravity field. The discussion has a bit of the handbook flavor with a detailed working out of all equations, including the specifications of the numerous numerical constants. The treatment could clearly serve as a text for the methods of celestial satellite geodesy. There is, however, no discussion of the relation of geodesy to other branches of geophysics nor of the numerous problems of the earth's interior that have been raised by detailed studies of satellite orbits.—*Gordon J. F. MacDonald*

Prehistoric Technology by S. A. SEMENOV, translated from the Russian by M. W. THOMPSON; 181 pages; $12.50; Barnes & Noble, 1964.

Thompson's translation of Semenov's work marks an important stage in prehistoric studies. Semenov attempts to identify the functions of stone implements and bone tools on the basis of wear patterns. This is done through microscopy and photomicrography. He thus can show the angle at which the tools were held, the direction of motion, and often presents a logical hypothesis as to its use, such as chopping wood, cutting meat or skin, etc.

Since the early days of research, prehistorians have worked almost exclusively within a typological framework, and most studies are arrested at this Linnaean level. Semenov's approach brings us nearer to understanding how man used these tools to cope with his environment. Changing typology may leave stratigraphic traces, but it tells us little about why man made certain tools, if more efficient ones were discovered, and what, in fact, he was trying to prepare or manufacture.

Unfortunately, Semenov deliberately ignores the reverse side of this coin, modern experimentation. His approach would be stronger if he could show us tools he himself produced in the fashion he describes, used in the manner he details, and bearing the telltale wear patterns he so admirably shows on prehistoric implements.

Stemming from this lack of experimentation, he makes a number of errors in his chapter on prehistoric manufacture. As an example, he states that wooden tools are too soft to produce prehistoric implements. In fact, one can produce an admirable hand-axe from a flint core using an ordinary hammer handle as a striker.

One minor fault in the book is a lack of scale accompanying photographs, which in themselves are excellent. It would also be important to know what percentage of each class of tools at each site shows the wear patterns he describes.

All archaeologists concerned with prehistoric implements should give this book careful reading and benefit from its excellent and painstaking approach, as well as the information this method yields.—*Henry T. Irwin*

Habitable Planets for Man by STEPHEN H. DOLE; 158 pages; $5.75; Blaisdell Publishing Co., 1964.

Here is an attempt to answer the question "What are the biological constraints upon man's survival in a planetary environment, and what is the probability that a given star can support habitable planets?" Though the query is old and proposed answers often resemble more a game than a science, this study by the head of the human engineering group of The Rand Corp. is the most thorough and quantitative available.

"Habitable" is first defined, then the temperature, illumination, gravity, water, atmospheric, and other requirements for producing a general ecology capable of supporting human life are reviewed in detail. This is followed by a chapter on general physical and chemical properties of planets, largely a review of the literature with several additional calculations. Next the mass, rotation rate, age, orbital characteris-

tics, and tidal forces necessary for habitable planets are examined and stellar binary systems treated.

To determine the probability of a star having habitable planets, the probabilities of satisfying each of ten necessary conditions are calculated—with varying degrees of convincingness. The author fully realizes the fragility of his numerical computations, but it seems silly to report to three significant figures results which are order of magnitude at best. This is followed by a detailed discussion of 14 promising stars within 22 light-years, whose combined probability of having a habitable planet is found to be about 0.43.

Three brief concluding chapters consider techniques of planet finding, types of habitable planets, changes which may be produced in man by differing environments, and a note on human destiny. These chapters also contain a reply to those who would dispute the wisdom of man in spreading his civilization throughout the galaxy.

The techniques and conclusions are of broad general interest and are understandable by nonspecialists, the style and format are pleasing, and a glossary and comprehensive bibliography are included.—*William C. Saslaw*

The Senses of Animals by L. H. MATTHEWS & M. KNIGHT; 240 pages; $7.50; The Philosophical Library, 1963.

Recent years have seen many exciting advances in our understanding of the sensory basis of animal behavior. Not only has there been continued accumulation of evidence that the senses may function very differently and be more or less important in other animals than in man, but sophisticated physiological studies have revealed several examples of unprecedented sensory capability and even the existence of entirely unfamiliar sensory modalities. The present volume is an attempt to capture the excitement of these recent findings in a brief and nontechnical summary of what is now known about the senses and their function at all levels of the animal kingdom.

In the first half of the book, Knight, a well-known British amateur naturalist, mentions a vast number and variety of interesting phenomena in a rambling discussion of simple field observations and experiments with everything from protozoa to man. Unfortunately, his chapters are repeatedly marred by careless writing and careless interpretation of observations. The result is often more misleading than informative.

The second half of the book, however, is entirely successful. Matthews, the Scientific Director of the London Zoo, writes with extraordinary ease and clarity. His chapters emphasize the physiological mechanisms underlying sensory function and the adaptation of the senses to particular modes of life in a wide spectrum of animals. Vision receives most attention, but hearing, touch, smell, and taste are discussed with a minimum of oversimplification. A separate chapter is deservedly devoted to the specialized use of sound for orientation by many animals, and the somewhat analagous use of self-generated electrical fields by certain fish. Another chapter deals with the sensory monitors of body position and internal condition, and with the mysterious phenomena of "internal clocks" and homing.

One of the few drawbacks of this part of the book is its lack of references and bibliography, making it difficult for readers to pursue interests stimulated by the text. For the nonscientist, however, or the student wanting a brief but accurate and comprehensive introduction to the senses, the second half of this book should prove a welcome find.—*Alan D. Grinnell*

Overtures to Biology, The Speculations of Eighteenth-Century Naturalists by PHILIP C. RITTERBUSH; 287 pages; $7.50; Yale University Press, 1964.

Overtures to Biology, as its title and subtitle suggest, devotes its attention to some eighteenth century speculations concerning natural history which were prevalent before biology developed in the nineteenth century into the experimental science that we know today.

The author considers mainly two primary patterns of speculations, separate but slightly overlapping. He takes up first electricity and the idea of immanence, which he defines as the belief that "interrelated subtle fluids caused all vital phenomena"; then, at somewhat greater length, he deals with analogies between plants and animals, as supposed explanation of the life of plants, and also in relationship to ideas concerning scales of beings and of functions. In a final chapter he discusses the roles of Erasmus Darwin, Lamarck, Sir Humphry Davy, and John Hunter as intermediaries (or otherwise) between the romantic speculators of the past and the more modern biologists whom the author would seem to consider to be more strictly empirical scientists.

Ritterbush is more deeply concerned with the speculations than with their observational or other factual foundations. He bases his analyses and syntheses on the writings of a far greater diversity of authors than will be familiar to most of his readers. His text is fully, but not over-obtrusively, documented; it is graced by wit and esprit rare in such scholarly writing. This brilliant book is recommended as stimulating and enlightening not only to biologists and historians, but to all readers of AMERICAN SCIENTIST who find pleasure in speculations about the speculators who were their intellectual forefathers.—*Jane Oppenheimer*

Chemical Background for the Biological Sciences by EMIL H. WHITE; 152 pages; $4.95 cloth; $1.95 paper, Prentice-Hall, Inc., 1964.

This book is one of a series on foundation of modern biology. The idea behind this series is that biology, a complex and diverse science, might well be taught from short monographs, each dealing with an important sub-area. The objective of this book is to provide the chemical background needed by beginning biology students, presumably before they are exposed to the usual chemistry courses.

A sound foundation in chemistry is essential for a modern biologist. Today, one finds an increasingly great emphasis on subcellular events; therefore, one rapidly enters the domain of biochemistry. Biochemistry is thus vital for an understanding of a very large part of biology. In turn, one cannot acquire a firm basis in biochemistry without a chemical background. Biology students often do not acquire even a basic knowledge of biochemistry early in their training. This is because they are first required to acquire a grounding in chemistry, and especially organic chemistry, which is often taught during the Junior year. Therefore biochemistry is relegated to the Senior year or even deferred to Graduate School.

This reviewer believes that a sound training in biology requires an understanding of the elements of biochemistry before the second half of the Junior year. The fundamentals of organic chemistry should be learned even earlier. Two ways are available for this instruction. One is to teach organic chemistry to sophomores. Alternatively, one can introduce organic chemistry briefly as part of a Freshman or Sophomore Biology course. It is for this purpose that the present book would seem designed.

The book itself consists of six chapters. The first two deal with the elements of basic chemistry, the next three with organic chemistry, and the last briefly discusses the chemistry of natural products. The writing is clear, and the selection of points to be covered is in general excellent. If a student understands the ideas in this book he will have a sound basis for further studies in biology. However, the material covered briefly here generally requires two years in the conventional chemistry curriculum. One could hope to gain from a short book at best vocabulary and major ideas. At a later date, this material should be supplemented by a thorough course in organic chemistry.

Such a brief book cannot cover all chemical topics valuable for biologists. But a few topics could have been expanded or treated more thoroughly. One of these, obviously, is heterocyclic

compounds, extremely important as components of vitamins and nucleic acids. Some understanding of their reactions, especially ionizations and oxidation reductions would be valuable to biologists. Another topic which might be discussed briefly is light absorption and spectra, in terms of structure.

All in all, this book provides a very good first step in the direction of bringing organic chemistry and biochemistry into the biology curriculum at the appropriate early stage. It should be useful both in teaching a beginning biology course and in telling the more experienced biologists about newer developments in organic chemistry.—*Arthur B. Pardee*

Advances in Physical Organic Chemistry, Vol. 2, edited by V. Gold; 288 pages; $10; Academic Press, 1964.

The volume contains reports on four topics in the field of physical organic chemistry—written by well known authors.

Clair J. Collins in his review on "Isotopes and Organic Reaction Mechanisms" discusses the different types of application of isotopes to studies of organic mechanism, i.e., the isotope dilution method, tracer studies (following the fate of a certain atom in a reactant molecule during the course of a reaction), measurements of reaction rates with the aid of isotopes, and kinetic isotope effects. He gives many selected examples of special interest to the organic chemist. The section on kinetic isotope effects mainly consists of an excellent discussion of the treatment of the experimental data which is not available in any other review article: the reaction rate method for the determination of isotope effects and its error limits, and the method of competing reactions.

The contribution of E. Whalley, "Use of Volumes of Activation for Determining Reaction Mechanisms" is the first review article which appears on this topic. It includes not only a survey of Whalley's own important work on the pressure dependence of the rates of acid catalyzed reactions and its mechanistic implications but it is also concerned with pressure effects on many other reaction types, i.e., unimolecular decomposition *versus* solvolytic processes, some rearrangements, Diels-Alder reactions, and radical polymerizations.

H. Zollinger, "Hydrogen Isotope Effects in Aromatic Substitution Reactions," gives a survey of the application of primary H-isotope effects to the evaluation of the rate-determining step in some aromatic S_E, S_N, and S_R reactions. The discussion is centered around the equation of the stationary state, with special reference to many interesting examples in which none of the consecutive steps alone is rate determining.

A. P. Wolf, in his contribution "The Reactions of Energetic Tritium and Carbon Atoms with Organic Compounds" devotes himself to the difficult task of reviewing a completely new field of work which refers to the reactions of "hot" carbon-11, carbon-14, and tritium atoms created by the Szilard-Chalmers effect. This review is of special value because this very important field is less familiar to the average physical organic chemist.

The volume may be recommended to everybody with a strong scientific interest in the field of physical organic chemistry. Since very little time has elapsed after the submission of the manuscripts by the authors, the book is completely up-to-date.—*A. V. Willi*

The Technology of Polyester Fibres by B. V. Petukhov; 89 pages; $4.50; The Macmillan Co., Pergamon, 1963.

The monograph aims to give a review of the main problems in the production of "Lavsan" (polyethylene terephthalate) fibers. As such, it is a course outline of published facts to 1960, the publication year of the first Russian edition. Without going into very much detail, a description is given of production methods for polyethylene terephthalate and its raw materials, polyester fiber spinning techniques, and polymer as well as fiber properties. Some attention is given to raw material specifications, the relative merits of different

polymerization techniques, and fiber treatments.

The main use of this monograph is, in the opinion of the reviewer, for those who are unfamiliar with the general area of polyethylene terephthalate production and fiber spinning. It is definitely not a handbook for workers in polyester technology. For this, it misses the depth and completeness of review and critical selection of experimental fact. The work can best be seen as an extended literature survey in the area of polyester technology. To take it as an authoritative treatise would be wrong and might lead one to repeat experiments which already have been repudiated elsewhere (for instance, the claims for Zn-acetate catalyst on page 16 are not so beyond question as one might believe—it has been shown that this catalyst promotes polymer degradation). Lack of definition of symbols used (such as N_m) is bad practice.

The translation is only fair. Although the text is clear in most places, one is continuously aware of the fact that this was translated from the Russian language. A mixed-up translation, as in the middle of page 45, is intolerable.—*H. J. L. Schuurmans*

Fatigue Resistance by P. Ye. Kravchenko; 122 pages; $5; The Macmillan Co., Pergamon, 1964.

The fact that this monograph is a translation from the Russian probably accounts for its having more prefatory sections than are usually found in books, even those with more monumental objectives. There is first a preface to the English edition by N. L. Day (who edited the translation) in which the work on fatigue in metals in Western countries is reviewed and the important literature cited. Kravchenko's references are all to Russian workers. Following this section is a foreword to the English edition, evidently by the author, in which he concludes with the indubitably commendable hope "that his modest work will help if only a little to strengthen contacts and friendship between our peoples." There follows another foreword, seemingly for the Russian edition, and an introduction serving as a brief historical review, before Page One of the main text comes into view.

The title of this volume, implying that it has to do with the fatigue of materials generally, is misleading. Actually the subject matter is the fatigue resistance of metals only. The monograph, intended primarily as a textbook for students at the undergraduate level, assumes the reader to have no previous familiarity with fatigue testing, and opens with a chapter on definitions and the graphical presentation of results along conventional lines. It thus serves as a helpful introduction to the subject, not only for the man interested in fatigue in metals, but (despite the limited orientation of the book) for the person interested in these phenomena in other materials. Other chapters on the external appearance of fatigue fractures, the theory of metal fatigue, factors affecting the endurance limit, and methods of design for improving fatigue performance have less broad application.—*W. James Lyons*

Physical Chemistry by D. F. Eggers, Jr., *et al.;* 783 pages; $9.95; John Wiley & Sons, 1964.

The domain of modern physical chemistry is no longer contained by "classical" topics such as equations of state, thermodynamics, kinetic theory, and their applications. Rather, a good basic knowledge of quantum and statistical mechanical principles is necessary for the understanding of a large number of topics. Numerous introductory texts exist in which both quantum and statistical mechanics are discussed with some rigor. But these subjects are usually treated more or less as separate entities, and the bridge between them and classical topics is not well defined and leaves much to be desired. In their text, Eggers, Gregory, Halsey, and Rabinovitch have succeeded in constructing such a bridge. It is new in concept and fresh in style, and should make a welcome addition to the library of the student and the researcher alike.

The book consists of eighteen chapters, which may roughly be grouped

into two parts. The first eleven chapters provide basic coverage of statistical mechanics, quantum mechanics, and thermodynamics, each of which is discussed with clarity and considerable depth; the remaining seven deal with applications of these principles in the description of solutions, transport and rate processes, chemical kinetics, molecular spectra and resonance phenomena, x-ray diffraction and crystal structure, reaction rates under nonequilibrium conditions and phenomena at interfaces. Despite the multiple authorship, the book is uniformly well written. A notable feature of the presentation lies in the continuity in thought and consistent permeation of basic principles throughout the text. Thus, the description of "classical" thermodynamics in Ch. 7 prepares the way for a rigorous treatment of statistical thermodynamics in Ch. 11. A fundamental knowledge of statistical mechanics acquired in Ch. 5 prepares the student for a better understanding of chemical equilibria, solutions, and chemical kinetics described later in Chs. 10, 12, and 14, respectively. Similarly, the opening chapters on quantum mechanics form the prerequisite for the discussions of molecular spectra and photochemistry presented in Chs. 16 and 17. The book is well-illustrated and well-studded with problems which complement the material covered in the text. Frequent reference to data from modern literature is helpful in emphasizing the application of basic principles.

Due probably to a conscious effort to prevent the text from getting too bulky, however, several specific topics are discussed in perhaps too brief a manner. For example, the derivation of Debye's T^3 Law for the heat capacity of monatomic solids should at least be outlined, even though the assumed frequency distribution for the solids is inaccurate. Also, the important, yet often neglected, topic of thermal conductivity could be treated at a greater length, in keeping with lengthy discussions of other transport phenomena in Ch. 13. A section on dielectric loss and microwave absorption phenomena could profitably be included immediately following the section on dipole moments and polarization phenomena in Ch. 16.

In general, this is an excellent text, and should present a challenge for the ambitious and promising university junior.—*Francis K. Fong*

Solids Under Pressure, edited by WILLIAM PAUL & D. M. WARSCHAUER; 478 pages; $15; McGraw-Hill Book Co., 1963.

"Solids Under Pressure" constitutes an excellent cross section of the field to the date of the publication. The introductory chapters by the late P. W. Bridgman to whom the volume is dedicated, and by A. W. Lawson might well serve to provide guidelines for those interested in initiating high-pressure studies in their own laboratories.

Subject matter of the remaining chapters provides a coverage of currently interesting topics on pressure effects in solids. These range from the highly detailed works on effect of pressure on properties of semiconductors in which the investigators have sought accurate knowledge of a wide range of effects in a relatively narrow class of very well characterized materials, to the investigations on phase equilibria and transformations in metals under pressures in which a specific set of effects was the principle target.

The final technique chapter is a "Review and Prospect" by Harvey Brooks, presenting a critical review of research in the field, with suggestions for both theoretical and experimental investigations to augment the existing status of understanding of solid state phenomena.

An extremely useful feature in this volume is an extensive and carefully classified bibliography of high pressure techniques containing a brief description of the contents of each article listed.—*W. B. Daniels*

Comparative Anatomy and Embryology by WILLIAM W. BALLARD; 618 pages; $10; The Ronald Press Co., 1964.

The subject of comparative vertebrate morphology has long been in need of an authoritative book designed for use in

undergraduate courses in which comparative anatomy and vertebrate embryology are presented in an integrated manner. The study of comparative anatomy achieves significance only when it is based on a good comprehension of both phylogeny and ontogeny; and the details of embryonic development, to be truly meaningful, require a knowledge of the adult structure of the organisms concerned. Yet, up to the present time, few books in the English language have been at all adequate for use in a coordinated course in vertebrate anatomy and embryology. The reason is not far to seek. The preparation of a treatise dealing with the broad sweep of the subject, accurate and up-to-date, and planned for the use of undergraduate students represents a formidable task. Professor Ballard in producing such a book has done an admirable job.

In its organization the book presents a somewhat unusual but highly commendable approach to the subject. After establishing a background of information regarding chordate evolution, classification and the major concepts of morphology, the author devotes a section of over one hundred pages to a consideration of the development of various types of vertebrates, up to what he chooses to call the pharyngula stage—the stage at which pharyngeal arches and pouches are prominent. Included in this section is a discussion of early morphogenesis, the establishment of fate maps, the early stages in the formation of the primary organs of the body, and finally an analysis of the major developmental concepts concerned. This section provides a ground work for an understanding of later developmental changes leading to the adult form; and, it provides, also, numerous examples of the manner in which normal and experimental embryology unite with phylogeny and comparative anatomy to form the basis for an understanding of the principles of comparative morphology.

Later sections of the book are devoted to comparative regional anatomy, again with the findings of developmental and adult anatomy well integrated. In these sections are numerous examples to show that the author has extensively consulted original sources. This is not a book which relies heavily on previous texts and in which old inaccuracies and faults are perpetuated, as is so frequently the case. As a single instance: in the chapter on the urinary system is presented one of the clearest and most accurate accounts of the comparative morphology of nephroi and their ducts available in any undergraduate text. This account includes information from recent experimental work and is free of the oft-repeated over-simplified statements regarding the succession of different types of kidneys in phylogeny and ontogeny. Many other examples of the attention given to accuracy and comprehensiveness and the manner in which conceptual aspects of the subject are dealt with could be cited.

Professor Ballard writes in an enviably lucid style. The large number of illustrations, many of them original and others redrawn from earlier publications, constitute an intimate part of the pattern of the book. This is a book which should find wide use in undergraduate courses—and, hopefully, it will stimulate the revision of old courses and the initiation of new ones in which comparative anatomy and embryology are more closely integrated than heretofore. It should be useful, also, in providing advanced students in other biological areas with a comprehensive view of present-day vertebrate morphology. And, it will be found a trustworthy source book by teachers of general courses in biology.—*Elmer G. Butler*

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